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Recent Developments in the Detection of Infra-Red Radiation

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DURING recent years there has been an increasing interest in radiation in the infra-red portion of the electromagnetic spectrum. The development of suitable sources, filters and means of detection for this region has lagged far behind the corresponding developments for the visible, ultraviolet, and x-ray regions. In fact, the far infra-red, from 30 to 1000μ, is still pretty much an undeveloped region for which very little such apparatus is available.

A considerable amount of research was carried out during the recent war toward improving both filters and methods of detection for the near infra-red, from 1 to 10μ, and this paper is intended to give a brief outline of the results of that work.[1]

Instruments for the detection of infra-red radiation can be divided into two main classes, *nonselective* and *selective*. Those in the first class, such as the thermopile, give a response that is proportional to the energy incident upon the detector, without regard to the way in which the energy is distributed as a function of wavelength. Those in the second class, such as the photo-cell, give a response that depends, usually quite markedly, on the distribution of energy with wavelength. The most important detectors in each class are as follows:

Nonselective	*Selective*
thermopile	photographic plate
bolometer:	photo-cell
metal strip	photo-conductive cell
thermistor	infra-red phosphor
super-conductor	
pneumatic cell	

The developments made of each of these types will be taken up in the order given.

The thermopile, which is very widely used as an infra-red detector, has not been greatly improved in sensitivity in the period of time dealt with here, probably for the reason that it had previously been brought to a high state of perfection. Some work has been done toward developing thermopiles with a rapid response, and this work has met with a good deal of success.[2] These devices are made by evaporating bismuth and antimony upon a thin film of cellulose nitrate.

A major contribution to the improvement of the performance of thermopiles has been made recently in the development of direct-current amplifiers for amplifying small differences of potential. This has had the effect of improving to a marked degree the stability and sensitivity of these detectors. Perhaps the best of these amplifiers is one that involves the use of two interrupters,[3] one in the input circuit and one in the

[1] A number of these results were first described at the March 1946 meeting of the Optical Society of America in Cleveland, and many of the references given here are to papers presented at that meeting. For details beyond the brief description given here one should consult these original papers, references to many of which are made in the present paper.

[2] L. Harris, *J. Opt. Soc. Am.* **36**, 597 (1946).

[3] M. D. Liston, C. E. Quinn, W. E. Sergeant and G. G Scott, *Rev. Sci. Inst.* **17**, 194 (1946).

output circuit. A tuned alternating-current amplifier is used to amplify the current from the interrupter in the input circuit. The second interrupter is synchronized with the first and is used to rectify the amplified signal. The noise level of this amplifier can be made extremely low, and it has been possible to detect with the amplifier thermoelectric differences of potential of the order of 10^{-9} v. Many of the modern infra-red spectrometers use this device, or modifications of it, for the observation or recording of infra-red spectra.

A *bolometer* is a detecting device that depends on the change in resistance of a material, usually a metal, with the change in temperature caused by the absorption of the incident radiation. The receiving element is placed in one arm of a bridge network, and the bridge is balanced with no radiation incident upon the sensitive element. When the element is then exposed to the radiation, the current resulting from the unbalanced condition is amplified and observed or recorded. Much of the effort in this field has been directed toward the development of the amplifiers, especially in reducing their noise level. It has been possible to construct amplifiers that introduce no noise that is appreciably above that inherently present in the bolometer itself owing to thermal fluctuations.

There are essentially three types of bolometer. In one type the receiving element is a metal strip. Advances in the technic of preparing these strips have been considerable. They are made by evaporating a thin film of gold,[4] nickel[5] or platinum[6] on a thin film of cellulose nitrate, the metal in each case being in the form of a strip a fraction of a micron in thickness. Such films have a very small thermal capacity and therefore a quick response to the incident radiation, the time-constant being of the order of a few milliseconds.

Another type of bolometer, called the *thermistor*, is made of a semiconducting material. The resistance of a semiconductor, instead of increasing linearly with temperature, as in a metal, decreases exponentially, and therefore much more rapidly, with increase in temperature. The thermistors are made of combinations of the oxides of nickel, manganese, and cobalt, and change in resistance about 4 percent per degree centigrade, about ten times the corresponding change for most metals. The oxides are used in the form of small flakes[7] from 1 to 5 mm long, 0.1 to 1 mm wide, and 0.01 to 0.02 mm thick, with metallic contacts at each end. Such an element has a resistance of the order of 1 megohm. The speed of response is about the same as for a metallic bolometer, but the sensitivity is somewhat higher.

A third type of bolometer involves the use of columbium nitride at the temperature at which it becomes superconducting,[8] which is about 15°K. The sensitive element is a strip of CbN, which is kept at this low temperature by surrounding it with a mixture of liquid nitrogen and liquid hydrogen. It has been found that the noise level varies with temperature in the transition zone, passing through several maximums and minimums in this region with variations in the noise level of more than tenfold. In order to achieve the highest sensitivity the temperature is kept constant at one of these minimums by passing a current through a coil surrounding the element. For an element of resistance 0.2 ohm the temperature coefficient of resistance in the transition zone is about 10 ohm deg^{-1}. This is enormously larger, relatively, than for other types of bolometer and makes this type a very sensitive detector.

One of the most ingenious of the detecting devices that has been brought to an advanced stage of development is the pneumatic cell,[9,10] now familiarly called the *Golay cell* after M. J. E. Golay, under whose direction the most recent modifications have been made. In the operation of this device the infra-red radiation is detected by the change in pressure of an enclosed gas upon which the radiation is incident. This change in pressure is used to move a mirror. Light from a source is sent through a mesh to the mirror and through another mesh to a photo-cell. A small change in the position of the mirror produces a

[4] C. B. Aiken, W. H. Carter, Jr. and F. S. Phillips, *Rev. Sci. Inst.* **17**, 377 (1946).
[5] B. H. Billings, W. L. Hyde and E. E. Barr, *J. Opt. Soc. Am.* **36**, 354 (1946).
[6] W. G. Langton, *J. Opt. Soc. Am.* **36**, 355 (1946).
[7] W. H. Brattain, J. A. Becker and H. R. Moore, *J. Opt. Soc. Am.* **36**, 354 (1946).
[8] D. H. Andrews, R. M. Milton and W. DeSorbo, *J. Opt. Soc. Am.* **36**, 518 (1946).
[9] R. A. Weiss, *J. Opt. Soc. Am.* **36**, 356 (1946).
[10] H. A. Zahl and M. J. E. Golay, *Rev. Sci. Inst.* **17**, 511 (1946).

large change in the light transmitted through the mesh arrangement, making possible a high sensitivity for this device. It is now being manufactured by a commercial organization and probably will find a wide range of applications.

Among the selective receivers, whose response usually varies markedly with the wavelength of the incident radiation, the photographic plate probably underwent less development during the war than any other. Again this may have been because it had been brought to a high state of perfection earlier. One development of interest is the extension of the idea of using a phosphor in combination with the photographic plate.[11] Here a layer of one of the recently developed infra-red-sensitive phosphors is placed next to the photographic plate. The infra-red light causes the phosphor to emit visible light, which is in turn recorded by the photographic plate. In this way spectra were photographed beyond 1.5μ, and doubtless the method can be improved to go beyond that value.

It has not been possible to extend appreciably the wavelength range of photo-cells into the infra-red, the limit remaining in the neighborhood of 1μ. One interesting development, however, has been made in the combination of the photo-cell with a phosphorescent screen in the now famous "sniper-scope." In this device electrons ejected by the infra-red from the photo-sensitive surface are imaged on a phosphorescent screen, thus enabling one to view a scene that is illuminated with infra-red light. The difference of potential between the two surfaces can be made several thousand volts, giving an image of quite high intensity. The resolution is also remarkably good.

Some excellent progress was made in the development of photo-conductive cells. It has been known for a long time that certain materials show an increase in conductivity upon illumination, and some of these materials are sensitive in the infra-red. Among them are thallous sulfide, lead sulfide[12] and selenium.[13] The sensitivity of the thallous sulfide and the selenium cell has a maximum at about 0.9μ and extends to about 1.5μ. The lead sulfide cell responds farther out, with a maximum sensitivity at 2.5μ and a threshold wavelength at 3.6μ. These cells have a very rapid response and are several orders of magnitude more sensitive than a thermopile.

One of the most interesting developments in this field was that of phosphors sensitive to infra-red. It had been known for a long time that certain phosphors show a quenching, or decrease in phosphorescent brightness, when exposed to infra-red, while others show a slight momentary increase in brightness under such an exposure. Attempts were made during World War I to use the quenching effect for infra-red signalling. During World War II more attention was given to the stimulation, or increase in brightness, as the means of detection, and work along this line met with a good deal of success,[14] the sensitivities being far superior to those observed for the quenching phenomenon. In fact, the sensitivities are comparable to those for the other methods of detection described in this paper. The phosphors that show this effect to a marked degree are: (i) the alkaline earth sulfides and selenides[15–17] to which have been added small amounts of a pair of rare earths such as europium, samarium, or cerium; and (ii) zinc sulfide[18] activated with lead and some other element such as copper or manganese. These phosphors must first be excited by visible or ultraviolet light, or by the radiations from a radioactive substance. They can be prepared in such a way that practically none of the stored light is released by the normal phosphorescence process, which is presumably caused by thermal vibrations of the crystal lattice. Upon illumination with infra-red light they glow very brightly, the light storage being of the order of 30 microlambert hr. In addition to their possibilities as detectors of infra-red radiation, these materials should prove interesting to study for valuable additions to our knowledge of the solid state.

It is probably not possible at this time to arrange the afore described instruments in order of sensitivity or speed of response. The more

[11] F. W. Paul, *J. Opt. Soc. Am.* **36**, 175 (1946).
[12] R. J. Cashman, *J. Opt. Soc. Am.* **36**, 356 (1946).
[13] G. K. Teal, J. R. Fisher and A. W. Treptow, *J. App. Physics* **17**, 879 (1946).
[14] B. O'Brien, *J. Opt. Soc. Am.* **36**, 369 (1946).
[15] F. Urbach, D. Pearlman and H. Hemmendinger, *J. Opt. Soc. Am.* **36**, 372 (1946).
[16] R. Ward, *J. Opt. Soc. Am.* **36**, 351 (1946).
[17] H. W. Leverenz and E. J. Wood, *J. Opt. Soc. Am.* **36**, 353 (1946).
[18] G. R. Fonda, *J. Opt. Soc. Am.* **36**, 382 (1946).

sensitive of them give a response equivalent to the noise of the apparatus for a signal of about 10^{-9} w. Most of them have time constants of the order of a fraction of a millisecond.

Although not directly connected with the afore described work, some mention should be made of the remarkable success attained during the same period with filters for the near infra-red. In general these filters are made by incorporating a dye with a plastic material such as Cellophane.[19,20] It has been possible to prepare a series of these filters with quite sharp cut-offs. For instance, some that transmit only 1 percent of the incident light at 1μ will transmit 75 percent at 2μ.

Most of the work described here was done under contracts between various research organizations and the Office of Scientific Research and Development and was undertaken because of its connection with the war effort. However, there is little doubt but that nearly all of the developments will find immediate application in research laboratories. It is to be expected that our knowledge of the near infra-red will be greatly extended by the use of some of these new technics.

[19] E. R. Blout, E. H. Land, W. F. Amon, Jr., A. Thomas and R. G. Shepherd, Jr., *J. Opt. Soc. Am.* **36**, 361 (1946).
[20] J. H. Shenk, E. S. Hodge, R. J. Morris, E. E. Pickett and W. R. Brode, *J. Opt. Soc. Am.* **36**, 569 (1946).

The Place of the Philosophy of Science in the Curriculum of the Physics Student

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I

IF we wish to exercise sound judgment about the success or failure of science teaching in the general scheme of our educational system, it may be a good idea to arrange a hearing. We may then listen to people who were compelled to submit to this teaching and later achieved such a great reputation in our cultural life that we cannot ignore their opinions about the contribution of science teaching to general education.

Ralph Waldo Emerson, who complained that science had become abstract and remote from human life, says in his essay, "The conduct of life":

> The formulas of science are like the papers in your pocketbook, of no value to any but their owner. . . . There is a revenge for this inhumanity. What manner of man does science make? The boy is not attracted. He says, I do not wish to be such a kind of man as my professor is.

If the goal of a teacher is to stimulate, by his own example, enthusiasm for his subject, Emerson's teachers of science seem not to have come very near this goal. It may be that Emerson was not a science-minded type. We may quote, therefore, a recent writer, who, besides his wide interests in all domains of human life, had a particular leaning towards science—H. G. Wells. He describes his reaction to his physics teachers in the Normal School of Science in London. He realizes exactly that what is wrong is not the ability of a particular teacher. "No man," says Wells, "can be a good teacher when his subject becomes inexplicable." He amplifies this statement:

> To a certain point it had all been plain sailing, a pretty science, with pretty subdivisions: optics, acoustics, electricity and magnetism, and so on. Up to that point, the time-honoured terms which have been crystallized out in language about space, speed, force, and so forth sufficed to carry what I was learning. All went well in the customary space-time framework. Then things became difficult. I realize now that it wasn't simply that neither *G.* nor *B.* was a good teacher. The truth, of which I had no inkling then, was that beyond what were (and are) the empirical practical truths of the conservation of energy, the indestructibility of matter and force, and so forth, hung an enigmatic fog. A material and experimental metaphysics was reached. . . .

H. G. Wells indeed laid a finger on a very sore spot in our science teaching. The "subject has become inexplicable" because the teachers themselves did not have training that put the emphasis on logical consistency and satisfactory coherence between abstract law and sensory

observation. Their upbringing was a purely technical one, with little regard for the role of science within our culture as a whole. Where physics teaching reached a domain beyond mechanical and electrical engineering, the subject was not satisfactorily explained because it was "inexplicable."

There is no doubt that the current interest in science has its origin not only in its technical application, but also in its bearing on our general world picture. Our generation is witness to the "relativity boom." At the peak of this boom, Einstein's theory was of hardly any use to the engineer. But, to express it perfunctorily, it changed our view about the "nature of time and space." The statement that concurrent with a given event here a certain event is occurring on the planet Mars, was formerly taken to have a definite meaning. We know now that such a statement can only be asserted "relative to a certain system of reference." This new doctrine appeared to add credit to the doctrine of the "relativity of truth" and seemed therefore to some people a "social danger," since it might contribute to a disbelief in the "absolute values" of ethics.

These problems have been haunting our generation for decades. But if we ask a trained physicist (not to speak of a graduate engineer) what his opinion is on these questions, we would notice immediately that his training in physics had not provided him with any balanced judgment. The scientist will, as a matter of fact, often be more helpless than an intelligent reader of popular science magazines. We face the same situation if we ask our graduate in physics whether the theory of quanta has justified the belief in the freedom of will or whether it has made a contribution towards the reconciliation between science and religion, as has been maintained frequently, even by scientists and philosophers of high reputation.

The great majority of these trained physicists —and by "majority" I mean more than 90 percent—will be unable to give any but the most superficial answer. And even this superficial answer will not be the result of their professional training, but the profit they have made from reading some popular articles in newspapers or periodicals. As a matter of fact, most of them will not even be able to give a superficial answer, but will just say "this is not my field, and that's all there is to it."

The result of conventional science teaching has not been a critically minded type of scientist, but just the opposite. The longing for the integration of knowledge is very deep rooted in the human mind. If it is not cultivated by the science teacher, it will look for other outlets. The thirsty student takes his spiritual drink where it is offered to him. If he is lucky, he gets his information from popular magazines or science columns in newspapers. But it can be worse, and he may become a victim of people who interpret recent physics in the service of some pet ideology which has been, in quite a few cases, an anti-scientific ideology. The physical theories of the 20th century have been interpreted, indeed, as having "abandoned rational thinking" in favor of—I don't know exactly what, as I cannot imagine what alternative exists to rational thinking in the field of science.

It is a fact which one may regret, but which is established empirically with no less certainty than the law of gravitation, that the science student who has received the traditional purely technical instruction in his field is extremely gullible when he is faced with pseudophilosophic and pseudoreligious interpretations that fill somehow the gap left by his science courses. I would even venture the statement that this gullibility increases in inverse proportion to the familiarity of the student with the conceptual analysis of science. I think that, statistically, the experimental physicist is more gullible than the theoretical physicist, and the graduate of an engineering school still more so.

For our purpose, it is sufficient to realize that the present type of science instruction does not enable the student to form even the faintest judgment about the interpretation of recent physics as a part of a new world picture. This failure prevents the science graduate from playing in our cultural and public life the great part that is assigned to him by the ever-mounting technical importance of science to human society. It is obvious that the necessity of teaching 20th century physics has made conspicuous the shortcomings in our methods of science teaching. It would be erroneous, however, to think that the

teaching of earlier, so-called classical, physics is not handicapped by the same kind of shortcomings. But, certainly, these deficiencies become the more conspicuous the more rapid the changes in physics that have to be presented in class.

If we look, for example, at the treatment of the Copernican conflict in an average textbook of science, we notice immediately that the presentation is far from satisfactory. In almost every case, we are told that according to the testimony of our senses the sun seems to move around the earth. Then we are instructed that Copernicus has taught us to distrust this testimony and to look for truth in our reasoning rather than in our immediate sense experience. This presentation is, to say the least, very misleading. Actually, our sense observation shows only that in the morning the distance between horizon and sun is increasing, but it does not tell us whether the sun is ascending or the horizon is descending. Starting from this fundamental mistake, the average textbook does not provide the student with an adequate picture of the historic fight of the Roman Church against the Copernican system. The student does not learn the way in which a powerful organization can oppose a doctrine established by science, and how this opposition can muster the support of reasonable and bright persons like the father of British empirical philosophy, Francis Bacon, who denounced the Copernican system as violating our common sense.

By its failure to give an adequate presentation of this historic dispute our traditional physics teaching misses an opportunity to foster in the student an understanding of the relations between science, religion and government which is so helpful for his adjustment in our modern social life. With a good understanding of the Copernican and similar conflicts, the student of science would have even an "inner track" in the understanding of social and political problems. He would be put at least on an equal level with the student of the humanities.

Let us now proceed with our hearing: having listened to Emerson and Wells, let us question an intelligent college student who is not a "concentrator" in science but took, say, a beginning course in chemistry or biology to round out his general education. We shall hear often of a deep dissatisfaction with these science courses as a contribution to general education. While such a student may take away from his courses in history or literature the impression that they have guided him into a wide-open field of human interest with an outlook in a great many new directions, he has had a very different reaction to his science courses.

We may take our example from the chemistry classes, where, according to an old tradition, the general outlook is more neglected than in the other sciences. In such courses, where science teaching is at its worst from the educational angle, formulas like H_2SO_4 are thrown upon the head of the student, and he will really feel, as Emerson did, that these formulas are "notes in a pocketbook, which are valuable only to the owner"—who is, in this case, the professional chemist—but without any broad human interest. The general student will get the impression that he sits in a show where formulas are tossed around in a kind of juggling game. This is, of course, a very inadequate impression, to which the student will inevitably be led by the purely technical approach in the teaching. He will probably not be much impressed if he is told that this juggling leads eventually to interesting results, perhaps in the field of solid and liquid fuels. For "fuel" is, after all, no broad human interest either, but obviously a concern for some specialists.

If, however, chemistry should be taught in a more human way, it would be clear that a formula like H_2SO_4 contains the history of mankind, the evolution of the human mind in a much more impressive and certainly more condensed way than does all the history of the British kings. What a good teaching of science has to achieve on behalf of general education is to bring out the heroic mental efforts of mankind, which are packed in the formula H_2SO_4 as in a nutshell, and to let the student live through the exciting historical and psychological experience that eventually found its abridged expression in such a formula. Then the "note in a pocketbook" will become a flaming manifesto to mankind.

II

The foregoing discussion raises serious doubts as to the ability of the traditional method of

science teaching in fitting science into the broader framework of human knowledge. For the scientist himself this state of affairs may be satisfactory or at least tolerable, provided that the student gets a good understanding of his own field, say physics. But I am afraid that the same shortcomings that have been obstacles in the past to fitting physics into a broader frame of reference may also be obstacles in the path leading to a satisfactory understanding of physics itself.

In order to check this apprehension, we have only to put to a student of science the following simple question, which is, in a certain way, a key question: What does it mean when you say that a geometric theorem—for example, the sum of the angles in a plane triangle is equal to two right angles—is "true"? The significance of this question was emphasized with the advent of non-Euclidean geometry. Even in a perfunctory treatment of non-Euclidean geometry the question will arise: What does it mean to say that our actual space is Euclidean or non-Euclidean? This question was regarded as very natural and pertinent by the founders of non-Euclidean geometry. It was, actually, a very common question when this new geometry was a novelty. But if, today, you examine a graduate in mathematics or physics, the majority will never have heard of such a question, and if you put it to them they will not understand its meaning without a very thorough interpretation. If we look into any current textbook of non-Euclidean geometry, which may have several hundred pages, hardly half a page will be devoted to the question of "truth", and these few lines are mostly an attempt to dodge the question. In most cases, the student will not even learn such a simple thing as that mathematics can prove only statements of the type: if theorem A is true, then theorem B is also true. But it can never prove whether A is true or not.

This failure of science teaching in the foundations of geometry has a devastating effect on the mind of the student. For if he does not grasp the exact relationship between mathematics and physics in this simplest example offered by geometry, later he will certainly be unable to understand precisely the relation between experimental confirmation and mathematical proof in the more involved domains of physics or, for that matter, in any exact science. This means that he is bound to misunderstand the role of mathematical theory in physics altogether. The failure of a satisfactory elucidation in such a fundamental matter has made it possible to find in a well-known textbook of college physics statements like this: Einstein proved "mathematically" that a material body cannot move with the speed of light. The student of physics is not even trained to get the instinctive feeling that no statement of physics can be proved mathematically, but that every "proof" in physics consists only in deriving mathematically one physical fact from other statements about physical facts.

By the failure to give a good account of the exact relation between mathematical proof and experimental confirmation, our traditional science teaching again misses an opportunity to teach the student a reasonable and scientific approach to all problems of human interest. For in all these fields the central problem is the relationship between sensory experience (often called fact finding), and the logical conclusions that can be drawn from it. The failure to grasp exactly the nature of this relationship accounts for the confused attitude of many people towards the complex problems by which they are faced in private and public life.

The role of mathematics and physics in the understanding of geometry is perhaps the simplest example by which the student can learn how to discern the role of facts and logical conclusions in the involved problems of human relations. As a matter of fact, every problem of physics where mathematics is applied gives us such an example. A simple and typical example is provided by Newton's laws of motion. The first law (law of inertia) and the second law look very simple, but they are a crucial issue for every instructor who teaches physics to beginners. Their apparent simplicity makes it easy to discover every confusion in the presentation, whereas in the treatment of an advanced subject of mechanics, such as the problem of three bodies in celestial mechanics, a little confusion can pass unnoticed.

It is not an exaggeration to say that 90 percent of the textbooks of physics on the college level present the law of inertia in such a way that its

meaning is obscure; it is formulated in words that are not applicable to any actual situation in the physical world. We learn mostly that a body upon which no force is acting is "moving along a straight line." But obviously the expression "moving along a straight line" has a physical meaning only if a system of reference is physically given, in which a straight line is fixed that serves as the model by which we judge whether a certain motion is rectilinear or not. But in the current textbooks of college physics one finds hardly even the perfunctory statement that the system of reference needed in the first law is the system of the so-called fixed stars. Hence, the statement of the law as it occurs in the textbooks has not even a vague meaning, for no method is described by which the validity of the law can be tested in a concrete case.

Since the method of testing remains obscure, one finds all imaginable opinions regarding the possibility and method of testing the validity of the law of inertia. In some books one reads that the law is self-evident and does not need any empirical proof; other authors, however, say that it is confirmed by the most familiar experience of our daily life; for example, a book lies quietly on a table if it is not taken off. This, again, is in contradiction to the assertion of some textbooks that the law of inertia is a hypothesis that cannot be proven by any experiment.

If one seeks an evaluation of this law of inertia as an achievement of the human mind, one finds not infrequently such statements as: it is amazing that it took so many centuries to discover such a simple and trivial law. It does not occur to these authors that this law may, after all, not be so "simple" as they believe, if so many great scientists were unable to discover it earlier. Obviously, to call it "simple and self-evident" means not to understand its real significance and not to give to the students a correct presentation.

The same kind of confusion is prevalent in the presentation of the second law. The mind of the student is usually so poorly trained in logical analysis that only rarely does he become aware that he cannot understand this law and asks for comment.

One of these rare cases has remained in my memory for years. A student in the ASTP asked the following question: We are told that a planet moves around the sun approximately in a circular orbit. The radius, in accordance with the D'Alembert treatment, can be determined by the rule that the gravitational force of attraction is in equilibrium with the centrifugal force. But, if there is no resultant force acting on the planet, how can it move in a circle? This man had not learned, as he should have done, that the words "equilibrium" and "rest" have to be used only with the specification "relative to a certain system of reference." Otherwise these expressions cannot occur in any physical law. A body is in equilibrium relative to an inertial system —for example, the fixed stars—if the "impressed forces" in the Newtonian sense—gravitation, cohesion, and so forth—balance one another. But a body is in equilibrium relative to a rotating system if the impressed forces balance the centrifugal force. This is the case that aroused the doubts of our ASTP student. The planet is at rest in a rotating system but "moves in a circle" with respect to the fixed stars.

While in the elementary textbooks the law of inertia is formulated in an elaborate but mostly meaningless way, many advanced textbooks take pride in minimizing the law. I even found a book in this category in which the law of inertia is called "self-explanatory." The author himself fails to explain it, relying evidently on the ability of the law to explain itself. In these advanced books the law of inertia is treated as a special case of the second law: acceleration is proportional to force. If the force is zero, the acceleration obviously is zero too, and the body moves with constant velocity. But the only distinction between this and what the elementary books say is very often that in the advanced book not the first, but the second law is formulated in a meaningless way. The result can be easily checked, if we ask any graduate student in physics what the word "acceleration" in the second law means. He will certainly know that the acceleration is the second derivative of the coordinates with respect to time; but when we ask him what is meant exactly by "coordinates," he will say that they refer to a Cartesian coordinate system. But a motion of a physical body is described only if we specify its coordinates with respect to a coordinate system that consists of physical bodies at which we can point. Therefore, to give an "operational" definition of what is meant by "acceleration" in the second law, we have to describe a particular physical Cartesian system. This certainly must not be in fixed connection with our earth. In the first ap-

proximation, we can take the body of fixed stars, our Milky Way, as such a Cartesian system. Then acceleration is, approximately, "acceleration with respect to our Milky Way." Most students of physics will be at a loss if asked a simple question like this. Even a candidate in the Ph.D. examination will sometimes answer that "acceleration" in the second law means "acceleration with respect to a Cartesian system the axes of which are fixed in space." Many will not even have a vague apprehension that the expression "fixed in space" may be meaningless in physics. But if one asks them a little more insistently, they will become confused and will guess that there is something rotten in their knowledge of the simplest laws of physics.

This failure to describe the physical system of reference to which Newton's laws refer does not do any harm to the student who takes mechanics for its use in mechanical engineering. For in this restricted domain we can work with the assumption that our own earth is the system of reference that is meant in the definition of "acceleration." In the extreme case, he will have to resort only to the system of fixed stars, as in computing the influence of the rotation of the earth on the orbit of launched projectiles. The engineer who has never heard of the difficult problem involved in the expression "acceleration" in Newton's law will by this failure not imperil the lives of people who use a bridge that was built according to his computations.

Obviously this state of happy innocence becomes obnoxious if it is preserved in the treatment of cosmological problems. The fixed stars, of course, are not at rest with respect to one another and do not exactly form a rigid Cartesian system. Therefore, they cannot be substituted in the Newtonian laws for the abstract term "Cartesian system." Rather, we must say that the positions and velocities of the fixed stars, and even of the remote nebulas, determine in some way the system of reference that the Newtonians mean when they use the term "acceleration" without any specifications. This means that the motion of a rolling billiard ball on earth is physically influenced by the positions and velocities of our Milky Way, and even by the galaxies that are millions of light years away from the earth. They determine the motion that we call "motion under no force." By ignoring the influence of the large but remote masses of the universe, the mysterious concept of "absolute space" was introduced into science. It remained there as a heritage of an ancient state of science which later was interpreted as metaphysics.

Obviously, as in the case of geometry, by the failure to give a satisfactory presentation of Newton's laws the teaching of physics misses a valuable opportunity. It could give to the student an example of the elimination of concepts that are left-overs from an earlier state of science and have survived in the disguise of metaphysical concepts. The "absolute space" and the "Cartesian system without physical background" can be proved to be superfluous for any logical derivation of observable fact. The student understands easily that they have no legitimate status in science, and are only the source of an odd verbiage about "real motions" in contrast to "apparent motions."

In the sciences that deal with human behavior the situation is essentially the same. One makes use of expressions like "real freedom" in contrast to "pseudofreedom," but the muddle around these expressions is very hard to disentangle. To learn how "real motion" was eliminated from science is a very good example of how to proceed in other fields. Hence, it has an educational value that can hardly be over-estimated. It paves the way for a correct analysis in the social sciences. But we are not to restate this issue of value for general education in this section. Rather, we shall stress the point that the same shortcomings which make the traditional presentation of Newton's laws unfit for general education, bar also its use for the understanding of physics itself, particularly of recent physics.

It is a fact which can hardly be overlooked that in the average college curriculum little attention has been paid to the theory of relativity compared with the great interest which this theory has met among the general public, and compared with the bearing of this doctrine on atomic and nuclear physics, not to speak of its key role in the logic of science. When, for example, the theory of the electromagnetic field is treated in the regular college curriculum, there are many teachers who skip the relation to the theory of relativity or treat it perfunctorily. They will tell

the student that "relativity" is an obscure theory which the student would hardly understand. By treating the subject in this perfunctory way they certainly contribute to the obscurity that may prevail in the mind of the student. He becomes convinced that the theory of relativity is fundamentally different from "sound" physics, such as Newtonian mechanics or Maxwell's electromagnetic field. He is likely to believe that modern theories are less logical and less closely related to the observed facts. The origin of this attitude must be found, certainly, in the insufficient training of teachers in the field of relativity. As a matter of fact, there is even a shortage of appropriate textbooks. There are good textbooks on relativity for advanced students and there are fairly good books for laymen, but a book on this subject for the undergraduate hardly exists.

It is easy to put the finger on the gap in the training of college teachers which produces their antipathy to a thorough treatment of relativity theory in the regular physics courses. The lack of precision in the foundations of geometry and mechanics at which we pointed does no harm in mechanical engineering, but becomes a major danger when we have to teach relativity. To the mechanical engineer the whole problem of the system of reference is trivial. He can get along splendidly with our good old earth as such a system. But when we go in for more general problems, such as motion with great speed, propagation of light, and so forth, we have to keep in mind constantly that these phenomena can be described with respect to different systems of reference. For some particular changes of the system of reference the laws of motion remain unaltered. This fact turns out to be the most important property of these laws, and is the very basis of the theory of relativity.

If the teacher fails to stress the role of the system of reference in ordinary mechanics, the student will later fail to grasp the gist of the theory of relativity. Moreover, if words like "absolute space" or "absolute rest in space" are not eliminated by adequate teaching of mechanics, the student will have trouble eliminating the concepts of "absolute length" or "absolute time" in relativity. Frequently, the teaching of traditional college mechanics has not been used to give to the student a correct understanding of the relation between mathematics and observation in science, but instead the metaphysical formulation of Newton's laws is hammered into the student's brain, as is done in most textbooks. Then the study of the theory of relativity will be a hard job for instructors and students. They will easily wind up in the pitfalls of metaphysical prejudices which are mostly covered by a thin stratum of common sense. They will believe that expressions like "absolute simultaneity" or "absolute length of a time interval" are immediately understandable to common sense. The result is, of course, that these students, on becoming instructors in physics, will never be able to get rid of the opinion that the theory of relativity is somehow contrary to common sense, or, at least, not as agreeable to it as traditional physics has been.

This situation is still worse in the approach to the quantum theory. As this theory is badly needed to organize experimental results in atomic physics, it would be hard to eliminate the quantum theory from the regular physics curriculum. But the teachers who do not feel on firm ground in the foundations of geometry and mechanics will teach the quantum theory as a collection of useful recipes, "quantization laws," which often take the form of "prohibitions." But they will dodge as much as possible the new general laws that have replaced the Newtonian science. The student will get little of Heisenberg's principles of indeterminacy or Bohr's principles of complementarity, which are the most relevant points in quantum theory for our general world picture. And if these points are touched, the same thing will happen as in the ordinary treatment of relativity. The student will be left with the impression that the new physics is somehow obscure and even "irrational." This perfunctory presentation of the new principles is, in some respects, even worse than the practice of restricting the teaching precisely to the formulas that describe actual observations. For the feeling that science has to be satisfied with an obscure theory makes the student an easy victim of quacks who exploit modern science in their endeavor to prove that we have to surrender our reason to a kind of blind instinct. This means in practice that we

have to surrender our good judgment to the control of people who are bold enough to pretend that they are in possession of that intuitive instinct.

To summarize these remarks: if the teacher of science takes it easy in the presentation of the foundations of geometry and mechanics, the consequences of this carelessness will leave imprints on the minds of the student that will make it very hard to train him later as a teacher of modern physics.

H. G. Wells, in his criticism of the science teaching he went through, says very correctly:

> The science of physics is even more tantalizing today above the level of elementary introduction (optics, acoustics, etc.). Brilliant investigators rocket off into mathematical pyrotechnics and return to common sense with statements that are, according to the legitimate meanings of words, nonsensical. Ordinary language ought not to be misused in this way.

This statement gives a good diagnosis of the disease by describing it as basically a "semantic maladjustment," as the school of general semantics would call it today. "Clearly," Wells continues, "these mathematical physicists have not made the real words yet, the necessary words that they can transmit a meaning with and make the basis of fresh advances." As we shall see in the next section, these words even hint at the remedy of the illness.

III

We have shown that our traditional way of teaching science is responsible for two deficiencies in the education of our students. First, not all instructors in science are up to the role that science, particularly physics, has to play in our general cultural life. Second, the 20th century theories (relativity and quantum theory) are still an obscure domain outside the well-illuminated field of common-sense physics. Uneasiness has not been removed by our usual teaching of physics.

If we now put the question of how to improve this situation, we know from the preceding discussion the sources from which the failure of our traditional teaching has originated. We know how it has happened that our science instruction is not successful in fitting science into our cultural life, and the modern theories into the frame of common-sense physics.

We discovered the main source in the failure to teach the students a clear distinction between the observation of facts and the drawing of logical conclusions. David Hume, the father of empiricist philosophy, has urged us to "surrender to the flames" all books that contain neither observed facts nor mathematical conclusions. This recommendation has become the basis of all attempts to make science a coherent and empirically confirmed system of knowledge. Accordingly, we have to teach our students how to eliminate all statements that are neither propositions describing the result of an observation, nor propositions that are part of a logical conclusion.

Therefore, we have to insist, above all, that the student learn to analyze the statements of his science in such a way that it becomes obvious what is a statement of observation and what is a logical conclusion. Many statements made in the traditional presentations of physics, particularly of recent theories, do not belong to either of the two types.

If we have carried out this analysis, it is convenient to present the result in such a way that from the wording of every statement it immediately becomes clear what its logical status is. For, in the traditional presentation of science, we are often unable to recognize whether a given statement is meant to be a statement about observed facts, or a hypothesis about facts that will be observed, or a statement that merely introduces new words or rules for construction of propositions.

If we try to single out the purely logical statements, we find them in every science. In geometry, for instance, a great part of scientific work consists in deriving conclusions from a given set of propositions called axioms. In drawing these conclusions, we do not need to know what the words in the axioms mean in the physical world. If the "points" and "straight lines" and "triangles" have the properties that are ascribed to them in the axioms, they also have the properties that are ascribed to them in the theorems, which we obtain by logical conclusions from the axioms. In the same way, in mechanics, if we start from Newton's equations, $ma=f$ and $a=\mathrm{d}^2x/\mathrm{d}t,^2$ we

can derive, by logical conclusion, a great many theorems. We find that if the force is zero, the coordinate x is a linear function of time, and if the force is constant, the coordinate x is a quadratic function of time, and so on. These statements are true whatever the meaning of "force" or "coordinate x" or "time t" may be in the physical world.

We obtain in this way purely logical statements. One occasionally calls them "tautological statements" because they tell us only that one and the same assertion can be expressed in different ways. If $ma=f$ and $f=0$, it follows that x is a linear function of t. But from this conclusion we do not learn anything about the physical world, but only to say one thing in different ways. By this perfunctory hint we learn already that more than a few failures in our customary science teaching have originated in the failure to recognize clearly the tautological character of these statements.

The world of physical experiments—briefly, the "physical world"—is described by statements about the procedure and results of physical measurements. They boil down, eventually, to statements like "a certain colored spot covers a second colored spot," or statements about a "pointer reading," as Eddington calls it. They consist only of words that are familiar to everybody. The description of physical experiments does not contain any element that is not used in describing our breakfast, and is therefore understandable to everybody who is trained to use the English language in his daily life. These statements have, obviously, a certain vagueness, and they are never completely unambiguous.

These "observational statements" are the second type of statement that we can single out in any science. In geometry, for example, we show how to measure the angles of a triangle and can find that the sum of the results is approximately 180°. In mechanics we measure the distance of a body from the floor of a room, and can find that in the case of a launched ball the distance from the floor is approximately a quadratic function of the angle traversed by the hand of a clock.

These two types, the logical and the observational statements, contain different types of words. In the first one, we have symbols like "points" or "forces" or "coordinates," which have no meaning at all in the physical world, while the second type contains words of our "kitchen language," which denote familiar things of our environment.

Customarily the student learns in science classes that by observational statements it is possible to confirm or refute the axioms of geometry or laws of mechanics. But, obviously, this way of speaking is somewhat superficial and logically incorrect. For the "observational statements" contain words of everyday life like "green," "hard," "overlapping spots," and so forth, while the "axioms of geometry and mechanics" contain symbols like "point," "circle," "acceleration," "force." A statement of the second type can never be confirmed or refuted by a statement of the first type, because it does not contain the same terms.

If science consisted only of principles and observations, its validity could not be tested by scientific methods. It is, therefore, of the utmost importance to teach the student that besides these two types of statements, science has to make use of a third type by which the language of the abstract principle can be translated into the language of observation. If the principles contain, for example, the word "length," one has to describe the physical operations by which the length can be measured. As these operations can be formulated in the language of everyday life, of observational statements, the description of the operation translates the abstract term "length" into terms of observational language.

These translating sentences have been studied with particular emphasis and care by P. W. Bridgman, and are called, according to his suggestion, "operational definitions." By this definition, an abstract term like "length" acquires "operational meaning." When these "operational definitions" are introduced into the abstract principles, the latter become "observational statements." Then we can check by actual experiments whether these observational statements derived from the principle, check with the observational statements that describe our direct physical experiments. It is necessary to understand that only in this indirect way can the principles of science be checked by experience.

Nowadays, the term "semantics" is often used

to denote the study of the relation between words and meaning. Hence, the sentences by which symbols are coordinated with observable things are called "semantic rules." They occur in the writings on the logic of science under different names. F. S. C. Northrop uses them as "epistemological correlations," H. Reichenbach as "rules of coordination," and so on.

This "logico-empirical analysis," or "semantic analysis," is a general method of "making our ideas clear." It is an efficient method for communication of thoughts and for influencing people. This analysis is the chief subject that we have to teach to science students, in order to fill all the gaps left by traditional science teaching.

If the student were taught this method effectively, he would be able to present and to understand recent physical theories—like relativity and the quantum theory—as precisely as classical physics, if—and this is the great "if"—if he understands ordinary geometry and mechanics precisely. Therefore, as "charity should start at home," so "semantic analysis should start in our own scientific back yard," at the foundations of our familiar geometry and mechanics. The application of logico-empirical analysis was never presented as clearly and concisely as in Einstein's paper, "Geometry and experience." He summarizes the results by saying: As far as geometry is certain, it does not tell us anything about the physical world, and as far as it makes statements that can be tested by experience it is not certain; that is to say, not more certain than any statement of physical science. By operational definitions "straight lines" can be translated into "light rays" or "edges of rigid bodies." Then the theorems of geometry become statements of optics or mechanics. Without these definitions, all theorems of geometry are tautological propositions about symbols.

Moreover, if we apply this type of analysis to mechanics, we shall naturally ask: what is the operational meaning of "acceleration"? If we want to translate this symbol into an expression consisting of observational terms, we have to describe the physical system of reference to which the coordinate x refers.

If we apply the same method to the theory of relativity, this doctrine will lose its isolated and obscure character. It will be obvious that "length" or "simultaneity" have to be defined by the description of physical operations by which these quantities can actually be measured. If yardsticks and clocks are affected by motion, it is equally obvious that the operational definitions of "spatial or temporal length" have to contain the velocity of the yardstick or clock used relative to a physical system of reference. This means that only "relative length" has an operational meaning, not "absolute length."

The reason why the theory of relativity has appeared so obscure to the physicists will then be very clearly understood by the student. Every alteration of the laws of physics seems understandable to common sense, if the semantic rules are unaltered. This would be the case if we replaced the wave theory of light by the corpuscular theory, provided that both theories remain within the frame of Newtonian mechanics. But the experiments on bodies moving with great speed suggest the introduction of new semantic rules. Such a change is much more fundamental and will appear obscure to the student of physics, if he is not, from the start, taught the role of these semantic rules in every domain of science, beginning with his high school plane geometry.

Bridgman rightly stresses the point that for a student who has studied relativity successfully, there is no need for a subsequent introduction to logico-empirical analysis. He could not understand relativity without it. The instruction in both theories is inseparable.

In a similar way, if we apply this analysis to the quantum theory, no student would fall for the talk that there is some "irrational" or "organismic" aspect in the quantum theory. He would understand that the very meaning of Bohr's principle of complementarity can be defined as the introduction of new semantic rules into the language of physics. As the alterations required by these rules are much more radical than those required by the relativity theory, the confusion of language has been tremendous. As our traditional teaching fails often to apply a correct logico-empirical analysis, a great many authors—scientists and philosophers alike—have failed to recognize that we have to do with a radical alteration of semantic rules. Many of them have even been caught by a wide-

spread propaganda by which the new physics of the 20th century has been exploited in the service of a campaign against the spirit of science, in favor of a confused "organicism," "spiritualism" or "irrationalism."

Clearly, by learning the application of semantic analysis, the student will not only profit by a better understanding of his own science, but also gain a more correct appreciation of the role his science plays in the frame of human culture in general. The close relationship of semantic analysis to modern physical theories on the one hand, and to the understanding of our general social and cultural life on the other hand, has perhaps nowhere been stressed so bluntly and sharply as by Bridgman, in a paper published in 1945.[1] He discerns the technical and the semantic significance of 20th century science, when he says:

> The second aspect of the modern epoch in science is, I believe, of incomparably greater significance. . . . He [the physicist] has come out with what amounts to a new technique of analysis, of great power and unexpectedly wide range of applicability. The new technique is applicable to all questions of meaning. . . . The potentialities of the new technique, when applied to domains outside the present application of science may be glimpsed by contemplating the confusion which now reigns. . . . It is becoming evident to an increasing number of people that an important part of our difficulties in analyzing and conveying meaning is of verbal origin. . . . There are popular discussions of semantics.

To appreciate this popular trend toward semantics, we need only look at such books as Hayakawa's *Language in action*,[2] which was a best seller. In *ETC: A Review of General Semantics*, edited by Hayakawa, semantics is applied to the problem of race relations, to labor problems, to the psychology of fear, and so forth.

Our point has been that an important step towards an improvement of our science teaching could be taken by consistent application of semantics or logico-empirical analysis. I do not believe, however, that this step would be sufficient. Even if such an analysis is carried out in a careful and competent way, there still remains much to be done if we want to bring out all the educational value that is inherent in science.

Once when I explained to a class the second law of Newton by the method of logico-empirical analysis, stating precisely the operational meaning of the symbols m, a and f, a student asked: Is this really everything contained in the law? Has the intuitive conception of a "force acting upon a body," which has played such a decisive role in the history of our thought, completely evaporated into the thin air of logical rules and physical measurements? As another example, everybody who has ever participated in discussions about the principles of physics occasionally encounters the person who becomes excited when the question is raised whether the concept of force can be eliminated from physics. At this point an emotional element enters the debate. The heat of the argument had its origin certainly not in science itself, but in the stretch of history of the human mind, which is now packed into the word "force." In the time when the Nazi party and its philosophy flourished, we could frequently find in German writing the view that the concept of "force" is somehow connected with the Nordic race. All people who wanted to eliminate it from physics were branded as enemies of the Nordic race. On the other hand, those who plead for the elimination of the concept of "force" regard it as a remainder of an animistic and anthropomorphic world picture, which even has a touch of spiritualism and superstition.

We learn from this example that one needs in addition to the semantic analysis a different type of analysis, in order to understand the role which the concepts and principles of science have played in our cultural life. We have to learn not only the operational meaning of symbols like "force" and "mass," but also how it has come about that just these symbols were chosen. Obviously the choice has not been unambiguously determined by their ability to form a basis for the derivation of observable facts. The symbols also have a life of their own. If we go in for this kind of research, we find that extra-scientific reasons have been largely responsible for the predilection in favor of or the aversion to some symbols. We learned it from the example of "force." A careful and thorough investigation

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[1] P. W. Bridgman, *Yale Rev.* **34**, 444 (1945).

[2] S. I. Hayakawa (Harcourt, Brace, 1941); also see W. Johnson, *People in quandaries* (Harper, 1946).

has shown that psychological reasons have played their part, as have habits carried since childhood, and even wishes emerging from the subconscious.

Besides these factors of individual psychology, the religious, social and political trend of the period have been responsible for the predilections and aversions of which we spoke. What we need, therefore, for an all-round understanding of science is an analysis of the psychological and sociological factors that went into the determination of our scientific symbolism. We may speak, briefly, of a "socio-psychological" analysis. We can summarize and say that this second type of analysis has to be added to the semantic analysis.[3]

Both types of analysis are parts of what one calls in more or less popular discourse "philosophy of science." Thus we may say that the remedy for the afore-described shortcomings in our traditional way of teaching science is to give more attention to the philosophy of science. More precisely, we mean by it logico-empirical and socio-psychological analysis, or, in the terminology of the school of thought which is known as logical empiricism: we need semantic analysis and "pragmatic analysis."

We have now to attack the more practical question of how the desirable instruction in the philosophy of science should and could be given to the students of science.

IV

In my opinion, the best solution is that the elementary science courses themselves should contain a good deal of the philosophy of science. This solution presupposes, of course, that there are a sufficient number of science teachers available who have adequate training in the philosophy of science. Speaking now in terms of "what should be," rather than in terms of "what is," I believe that no teacher should give an introductory course in science if he lacks training in semantics and the history of science. I realize that this solution is not feasible today. Hence, my second choice is to introduce courses in the philosophy of science which are to be obligatory for the students of science, at least for those who are later to become teachers of science. In a certain way every scientist is a teacher of science in his environment, whether he works as an engineer, a physician, or even a pharmacist.

Courses in the philosophy of science for the benefit of science students have been given rarely hence there is no coherent tradition. We are faced with the problem of building up such a tradition. There are two reefs that have to be avoided. The greatest weakness of the attempts which have been made occasionally to give such courses has been the lack of any definite conviction on the part of the professor. We must never forget that the word "professor" is derived from "profess," and this holds true, particularly, for a teacher of philosophy. His work with the students should be a "profession" in both meanings of this word. Scientists who have been teaching the philosophy of science have mostly offered a kind of incoherent digest of philosophic opinions. The choice has been mostly determined by the chance acquaintances of the teacher. The students have remained unimpressed by such courses or books. We meet this eclectic attitude even in the writings of such excellent scientists as Jeans or Planck. I remember the lectures of a great physicist, Boltzmann, on the philosophy of physics, which I attended as a student. Despite the personal greatness of the lecturer, the effect of the course was slight, because of a lack of a coherent approach. We can notice, on the other hand, that scientists who built their books around a central idea have shaped the minds of the science students for decades. I mention, just as examples, Mach, Poincaré and Bridgman.

Although a coherent approach is the first requisite for successful instruction, the second reef on which our course could be wrecked is a narrow-minded indoctrination. I remember that an intelligent girl student told me once: " It is a very unpleasant feeling to have a certain doctrine drilled into your brain without being provided with an outlook into the open field of widely divergent opinions." These two requirements of a coherent approach and of an open field seem to exclude each other. But this is true only superficially. By adding the socio-psychological approach, it becomes clear that every opinion which has been advocated in history has its legitimate place within a coherent presen-

[3] Jean Piaget, *Les trois conditions d'une épistèmologie scientifique. Analysis* (Milan, 1946) Vol. 1, No. 3, p. 25.

tation of science, based on logico-empirical analysis.

Our problem now is, practically, to build a course in which the student gets training in both types of analysis; a course that not only fills the gap left by the traditional science teaching, but also leads into the wide field of controversial ideas for bridging this gap.

For six years I have given a course at Harvard with this goal in mind. I have tried to improve it from year to year according to my increasing experience with students. I will describe the content of this particular course because of my familiarity with it, and not because I believe that it necessarily represents the best possible course. The course takes two terms. Actually, I give two half-courses which are designed for students of different backgrounds. But in this description I am going to integrate both half-courses into one coherent presentation.

My starting point has been the traditional distinction between "scientific truth" and "philosophic truth." Specifically, I have referred to the formulation that the great medieval philosopher, Thomas Aquinas, gave to this distinction. According to him, there are two criterions of truth: the first requires that a proposition can be logically derived from "evident principles" (philosophic truth); the second requires that the logical sequences of the proposition in question can be confirmed by experience (scientific truth). Obviously, if the truth of the principles themselves can be tested only by comparing their logical consequences with experience, the two criterions merge and we have only one criterion, namely, the scientific one. Then the traditional distinction disappears. This is the view that is called "empiricism," or "positivism." To assume the distinction obviously means to believe in principles the truth of which can be checked by extrascientific methods. In modern science this distinction is no longer upheld as an absolute distinction, but it has always played a certain role in a weakened and "relativized" form. We still say that there are propositions of physics which can be "proved" if we use the traditional language of physics textbooks. This means that they can be derived from general principles (like conservation of energy) which we hold to be true with a high degree of plausibility. Distinguished from these are propositions that are records of direct observation and cannot be "proved."

After these introductory remarks, a historical survey is given of the principles that have actually been used by the scientists as the basis from which the propositions of physics have been logically derived. There is one period in history that we can study from the origin to the end. This is the period of mechanistic physics, which lasted approximately from 1600 to 1900. Its basic principles were Newton's laws of motion. A proposition of physics was regarded as "explained," or "understood," or "proved to be true," if it could be logically derived from Newton's laws of motion. But the question arose, of course, whether Newton's laws, themselves, were "evident principles." The belief that they are, arises very often among students of physics as a result of the perfunctory way in which these laws are presented in the traditional textbooks. The only effective way to destroy this belief is a thorough presentation of the period before 1600, which preceded the era of mechanistic physics. In my course, medieval and Aristotelian physics is presented and analyzed carefully. I call it "organismic physics," because the basic principle is the analogy between physical phenomena and the behavior of organisms.

If the student has become aware that there was a time when reasonable people did not believe in Newton's mechanics, it will not hurt his "common sense" that after 1900 again a period could start in which these classical laws of motion were modified radically. By becoming familiar with a conception of physics that prevailed before the period of mechanics, the student will look at the disintegration of mechanistic physics around 1900 as a phenomenon analogous to the disintegration of organismic physics around 1600. In both cases, before the actual revolution took place under the impact of new discoveries of facts, the belief in the certainty of the ruling principles was shaken from within by logically minded critics. In medieval physics this role was played by the school of nominalism (Occam's razor), while the criticism of mechanistic physics came from the "positivists" of the last quarter of the 19th century, from men like Stallo in America and Mach in central Europe.

The rise of 20th century physics, of relativity and the quantum theory, was closely connected with a new view of the basic principles. It was no longer taken for granted that the principles from which the facts had to be derived should contain a specific analogy, either to an organism or to a mechanism. Nothing was required except that the observed phenomena could be derived from the principles in a consistent way and as simply as possible. The words and symbols that occurred in the principles, and the way these were connected, could be invented according to their fitness as bases for deriving the phenomena discovered by the experimental physicist.

This means that the symbols used in the principles had in themselves no meaning beyond their value for the derivation of facts. From this introduction of abstract symbols arose the obvious need for including in the theory prescriptions for relating these symbols with sense observations. The necessity and the importance of these sentences was particularly emphasized and elaborated by Bridgman in his requirement of "operational definitions." This new aspect of physics, which dismissed any traditional analogy and stressed only the criterion of empirical confirmation and logical coherence, may be called "logico-empirical physics."

Under this name I refer in the course to the physics of the 20th century, and dismiss by this terminology all misleading metaphysical interpretations of relativity and quantum theory which we owe to philosophers, sociologists, theologians and—I am sorry to say—also to some physicists.

After this historical framework has been established, I proceed to the application of logico-empirical analysis to the most relevant parts of physics. The start is made with geometry, where the situation is easier to explain than in any other field. I believe that the logico-empirical analysis of geometry provides the student of science with the best introduction to the philosophy of science. I would even venture to say that, if there is little time available, the familiarity of the science student with the analysis of geometry would suffice to fill, in a fairly satisfactory way, the gap left by traditional mathematics and physics teaching. By this analysis the student will learn that mathematics cannot prove any facts, but can only derive facts from other known facts. He will learn that there are two fields of science that we call "geometry." "Mathematical geometry" has the described tautological character and cannot teach us anything about the "nature of space." He will learn that only by the introduction of operational definitions—for example, "a straight line is a light ray"—can theorems of geometry become statements about facts and can be confirmed experimentally as any law of physics. He will learn that no geometric theorem by itself can be confirmed by experience, but only a geometric theorem—such as the one about the sum of angles in a triangle—plus the operational definitions, of straight lines, and so on. He will learn that statements like "the axioms are self-evident" or even "they are true" are meaningless, if no operational meaning is given them.

I would even venture to say that a thorough semantic analysis of geometry contributes more to the intellectual outlook of the student than a superficial philosophic interpretation of the whole field of physics. By this analysis of geometry the student will be introduced to the new logical technics in modern physics. Bridgman says of these technics that their impact on the pattern of our culture will eventually be greater than that of the technical consequences of modern physics, including atomic energy. The logico-empirical analysis of geometry will, moreover, enable the student to form a sound judgment about the "truth" of non-Euclidean geometry, and in this way fill a gap that yawns in most textbooks. The problem of "truth" is usually dodged in the presentation of non-Euclidean geometry, so that this part of mathematics has remained an obscure spot to most students.

Then the course passes to the analysis of Newtonian mechanics. From the beginning, the operational meaning of terms is stressed. This means, above all, that the role of the system of reference emerges clearly. Newton's laws are presented not as self-evident, but as paradoxical, for so they appeared to Newton's contemporaries.

We proceed then to the mechanical theory of light. When interaction of light propagation with the motion of large material bodies occurs, and both phenomena are assumed to follow Newton's

laws of motion, one can predict phenomena that are in contradiction to observed facts, as in Michelson's experiment.

This contradiction is the origin of the restricted theory of relativity, which is analyzed thoroughly with the emphasis on the logico-empirical aspect and on the place of operational definitions. Then all those misunderstandings disappear that have made this theory "obscure" and "contradictory" to common sense. It becomes obvious that the role of the "observer" is not different from his role in Newton's physics. No subjective element occurs in physics and, hence, there is no argument in favor of idealistic philosophy. The revolutionary changes brought about by relativity consist, first, of new observable facts, such as the change of the rate of clocks by motion, and second, in the suggestion which Einstein made to alter the language in such a way that these new phenomena can be described in a simple and practical way. The relativity of time, for example, is no advance in metaphysics, but in physics and semantics.

In the same way the quantum theory is analyzed. The famous relation of indeterminacy has nothing whatever to do with the introduction of a subjective or spiritual factor into physics. It is actually a suggestion for introducing a language that best fits the facts. Since there is no law of physics that incorporates the expression, "the position and velocity of a small particle at one and the same time moment," there is no reason to introduce such an expression into the language of physics. Bohr's principle of complementarity provides the suggestion that our physical world be described in a complementary language.

The course then proceeds to a logico-empirical analysis of the conception of causality, determinism, indeterminism and chance along the same lines. Eventually the concepts of mass and energy are analyzed.

After this description of the way in which semantic analysis is actually applied to physics, the course turns to problems of what has traditionally been called "philosophy." The first, historical part was a survey of the way principles of science have developed from the organismic to the mechanistic physics, and further to the logico-empirical stage. In this stage there was no demand for a specific type of principle or a specific form of words in the principles. The wording of the principles became irrelevant from the viewpoint of science itself. But there have been extrascientific reasons for insisting on principles of a certain type. These reasons include mere sluggishness in changing old principles or, very often, theological, ethical, or political factors. It has been argued that the principles of science have to be in agreement with the prevailing principles in those extrascientific fields. We remember the Thomistic distinction between philosophic and scientific truth. This distinction is based on the belief that one can assume some principles for reasons which are not in agreement with their consequences as experienced. This means that one assumes these principles for extrascientific reasons. Every system of thought that has this belief as its basis is a metaphysical system. Since in these systems the principles are closely connected with the religious, moral and political predilections of a certain period, the study of the metaphysical foundations of science is important for the understanding of the position of science in our cultural life. Therefore, the next part of the course is devoted to the metaphysical and antimetaphysical interpretation of science.

The most important point, as it seems to me, is to get the student to understand the extremes. I take pains to present an adequate conception of "straight metaphysics" and, at the other extreme, "straight positivism," which bluntly says that there is *no* principle except those which can be confirmed by the agreement of their consequences with experience.

As the example of straight metaphysics, the Thomistic philosophy is presented at length as far as it serves as a basis of science. The students are encouraged to read a textbook of "cosmology" that is currently used in Catholic colleges and contains the Thomistic foundations of physics. Once I asked a teacher at a Jesuit college, who was taking my course, to explain to the students the authentic teaching of Thomistic philosophy.

Then I proceed to the other extreme, which denies the distinction between the two kinds of truth and recognizes only scientific truth. I give a short historical survey starting with David

Hume, followed by a short account of A. Comtes' "positive philosophy" and a more elaborate discussion of positivism at the end of the 19th century; of the Americans, Stallo and G. S. Peirce; of the Europeans, Mach and Poincaré. Passing to the 20th century, we discuss the "operationism" of Bridgman and its relation to the pragmatism of William James and Dewey. Bridgman's emphasis on the role of language leads us to the last phase of positivism, the logical positivism of Wittgenstein, Carnap, and others.

After the presentation of straight metaphysics and positivism, we discuss some attempts to produce a reconciliation between both views. There are scientists and philosophers who present science generally in the positivistic manner, but reserve a separate compartment for metaphysics. Among them are Spencer, Fiske and P. Duhem. Systematic attempts at reconciliation have been made on the basis of idealism and materialism. The most powerful idealistic attempt is the school of thought founded by the German philosopher, Kant. Proponents of this school claim that there are self-evident principles, but that they are actually statements about our own minds. By making statements about geometry, we actually make statements about our own way of describing nature. Therefore, these statements are, in a certain sense, checked by experience, but not by external experience. We observe ourselves and believe that we observe the external world. This Kantian compromise has, as an outstanding representative among the scientists of recent days, the British astronomer, Eddington, whose views are discussed elaborately in the course. There are a great many varieties of Kantian idealism. They fill the whole spectrum of opinions between positivism and metaphysics.

The other important scheme of reconciliation is materialism. In its original form it was a generalization of mechanistic physics, with no metaphysics in it. Later it became obvious that the phenomena of life and of human behavior could not be interpreted easily in terms of Newton's mechanics. But a great many people felt the relevance of keeping to the mechanistic scheme. In contrast to mechanistic materialism, Marx and Engels developed the system of dialectical materialism, in which "matter" no longer means the matter of mechanics, but a substance which possesses all the qualities that are needed to account for the evolution of human life, individual and social. The importance ascribed to the principles of this system often went beyond their value for the description of observable phenomena, and the possibility of any change in these principles was minimized. This attitude brought a bit of metaphysics into dialectical materialism. After it had become the official philosophy of the Soviet Union, the relevance of the principles themselves was bolstered by extrascientific reasons. Dialectical materialism is discussed in the course in its application to the foundations of science. As in Kantian idealism, we find in dialectical materialism all varieties from almost pure positivism to almost pure Hegelian metaphysics.

These discussions of the philosophic interpretations of science are of great importance to the general education of future scientists. The interpretations should not be neglected in the teaching of the philosophy of science, for they are the link connecting science with the humanities. They provide the instrument that is used by religious, ethical, and political creeds to muster the support of science. F. S. C. Northrop emphasized in his recent book, *The Meeting of East and West*,[3] the great importance of the philosophy of science for any ethical or political creed. To understand this connection, we have to understand the philosophic interpretations of science and the link of metaphysical creeds with religious and political creeds.

The last part of the course is devoted to the description of these ties. It has become almost a commonplace that the communist and other left-wing creeds have their philosophic basis in materialism, while the right-wing groups look for their foundation mostly to some kind of organismic metaphysics, for example, to Thomism. It is, therefore, very important that the student have a good training in these philosophic interpretations which have become the bases of political creeds. The combination of philosophic and political creeds is often referred to as "ideology." The student of science does not

[3] F. S. C. Northrop (Macmillan, 1946).

need to be ignorant of this important field. He can take science and its interpretations as his door of entrance. For this reason, in the course in philosophy of science much attention should be paid to these philosophic interpretations of science that have been the basis of ideologies. The most elaborate of these interpretations are Thomism and dialectical materialism. Prominent Cardinals of the Roman Church, as well as prominent political leaders like Lenin, who were able to look under the surface of things, have taken pains to introduce into the teaching of science an interpretation that is favorable to their creed. Hence, the future scientist should be taught to take advantage of these ties and get a real insight into historical and contemporary ideologies.

The discussion of these ties helps to make the course in the philosophy of science relevant and attractive to the student. Moreover, this way of presenting world problems makes it possible to include a discussion of the relations between science and religion—a subject often regarded as too delicate for a sincere presentation, and so either omitted or presented in a conventional way. On the other hand, I have noticed that students are extremely interested in this topic, and ask embarrassing questions if it is treated in an evasive way. If religion is discussed among the ideologies, the treatment can become sincere, precise and inoffensive. It will become clear that in this question also the best way of approach is from science through its philosophic interpretation.

This is one possible outline of a course on the philosophy of science. The choice which I made among a great many possibilities is, of course, determined by my own background, scientific and personal. A great many paths can lead to the same goal. But all possible courses must, in my opinion, be based on logico-empirical and socio-psychological analysis of science. Only by this method can we steer our ship between the two reefs of overspecialization and superficiality.

Western Culture, Scientific Method and the Problem of Ethics*

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THE endeavor in this paper is not to solve problems but to state a point of view. For emphasis, this has been done fervently, somewhat dogmatically, and without great circumspection. The analysis of Western culture is certainly incomplete and probably exposes traits common to all cultures. Many of the points here made have recently been presented more forcefully and with better documentation by Professor F. S. C. Northrop.[1] The last part of this paper, however, departs markedly from Northrop's thesis. In it I take a position which will, I fear, incur the criticism of many readers, since it affirms the belief that ethics is a verifiable discipline and that its formal structure should be that of a science. But my intensity of conviction on this point has grown rather than diminished as a result of discussions with fellow scientists and others.

I.

1. Among the points of view from which a culture can be analyzed, we select a rather unpopular one, a view that places scientific method in the center of interest, leaving philosophy, esthetics and religion in the background of the cultural scene. The selection is made because the scientific element is believed to be the characteristic *par excellence* of Western culture. In the following I hope to make clear this assertion and to provide evidence for it.

Certainly, its truth is not obvious, for Western

* The first part of this paper, which deals with the function of scientific method in Western culture, was presented at the Conference on Science, Philosophy and Religion at its Seventh (1946) Meeting in Chicago. At the suggestion of participants in this Conference the later portions, concerned with postulational ethics, have been added.

[1] F. S. C. Northrop, *The meeting of East and West* (Macmillan, 1946).

culture existed apart from others before the advent of modern science. Hence what must be shown is that the significant traits both of our culture and of modern science spring from similar roots. For we do not wish the assertion to be understood as merely saying that technological achievements have molded the pattern of our external lives. This is a commonplace and would be of no significance in the present discussion if our science were but an incidental factor, powerful but inessential, as it is in some civilizations. Rather, the claim that our culture is typically scientific is here made together with an admission that a way of life wholly dependent on the appliances of science may not possess this character. The Japanese were an example of this apparent paradox.

Endless debates can be argued about whether our culture is this or that, and the results, while edifying, are often fruitless. Such formal debate is not intended here. If no fundamental insight leading to possible action could be gained from an inspection of the present thesis, our interest in convincing the reader would be very small indeed. But there are practical issues of great magnitude which seem to demand considerations of the sort to be presented.

Even at the risk of giving offense, it seems proper to say that history and anthropology have hitherto been disciplines largely marked by creative impotence, except in so far as they give satisfaction to scholars. Perhaps there is no reason why they should be creative in the sense we wish to convey, a sense that has not reference to creation of new knowledge (this is indeed achieved by history and anthropology) but to the establishment of principles for prediction (where they fail). Among the reasons for this deficiency is the desire on the part of the historian to remain in the realm of *facts*; indeed he deems it *unscientific* to go behind facts and to delve into theory. But at this level the essence of our culture cannot be caught: *analysis*, guided by rules of tried effectiveness and grounded in fact, needs serious consideration in the solution of the cultural problem. And the possible gain is considerable. Recognition and study of the mainsprings of our culture, made possible by analytic procedures familiar from science and philosophy, carry the promise of converting history into a fertile enterprise, capable of warning and predicting. The least we may expect from the approach to be sketched in this paper is the discovery of conditions foreign to the basic attitudes of Western life, conditions that inaugurate the breakdown of our culture. The most we can hope for, having gained this requisite understanding, is the wisdom to avoid impending disaster.

To illustrate the magnitude of what may be lost by the use of ordinary historical perspective, and what may be gained by the type of analysis here attempted, let us face the terrifying problem of our day. Those who know the scientific facts concerning atomic energy seem convinced that only a form of world government can save life on this planet. Historians and political scientists discourage and belittle the utopian suggestions that physicists have made. Their verdict, based on a study of cultural excrescences of the past, makes world government appear impossible in the near future. The questions we would raise are these: Whence does history suddenly gain predictive power? What is the logic of the argument which concludes from the absence of world federation in the past its impossibility in the future? The urgent query is clearly: What are the permanent, ideal, methodological tenets of our culture, and is a world state compatible with them? Thus the kind of analysis to which we direct attention moves into focus significantly.

2. Inferences cannot be drawn from facts, nor are facts ever pregnant with prediction. Inference and prediction require first an *interpretation* of facts: Only a theory about facts renders inference possible. In the following this will be made clear with respect to scientific method. A realization of this crucial condition is necessary at the very beginning of our discussion. To understand culture it is necessary to see what general view of the world and of life is most compatible with the data it presents, and to ponder over its implications. Strangely enough, the layman sometimes regards this excursion beyond data as unscientific and dangerous. Scientists find it difficult to understand that attitude, for they know that the more highly developed a science, the more freely, and the more successfully, does it practice ultrafactual

interpretation. We begin, then, by *interpreting* Western culture.

I doubt whether an impartial observer of our civilization could fail to be impressed by its strong emphasis on *stable patterns*; indeed it would seem that, if there be any striking difference between East and West, it expresses itself in such emphasis. We wish for ourselves a certain permanent form of government and tend to consider it as the optimum political arrangement for any nation. We want law and order; we want our jobs secure. We save, and make arrangements that will allow us to control our lives, even in old age, in a manner likely to produce a minimum departure from preceding stages. Our annoyance at some political and social events, and at the weather, arises from the circumstance that they are not completely predictable. We insist on the same sort of justice for all, and our mores tend toward great uniformity. Our various languages have been frozen into crystalline states around lattices of grammar and standards of usage, and we enforce mass education to level off differences of knowledge. When we come to express our religious convictions they tend to form creeds, and in philosophy we announce systems and general *Weltanschauungen*. In our daily lives the phenomena of industrial standardization take on increasingly greater force, and even our tastes for enjoyment, proverbially so divergent, approach a norm.

Others, while accepting this general diagnosis, might see the prevailing feature of Western culture not so much in a search for stability as in emphasis on rational form. Indeed, all the descriptive pronouncements of the preceding paragraph can be explained by assuming that our public life confers attention upon the rational elements of existence, much to the exclusion of the emotive or the esthetic. Doubtless this interpretation goes quite as deeply into the situation as does the other; if causal reasoning were *a propos* in this analysis one might prefer to say that rational form prevails because it is the agency which invests observable patterns with stability and permanence.

Nowhere does search for permanent pattern attain higher intensity, or reach greater success, than in theoretical science. It is for this reason that the term *scientific* so clearly characterizes our culture. It is meant to label its roots, not its external make-up. Basically, we believe in freedom for the same reasons that we accept the theory of the atom; both doctrines offer maximum opportunities for comprehending large portions of human experience in a uniform way. Whatever has given Western culture its uniqueness, whatever impels it to move in a specifiable direction, that same force becomes articulate in scientific theory. Hence its basic features are manifest, if anywhere, in the methodology of science, and they can best be studied there.

The term science must not, however, be construed too loosely; it should signify what is more definitely called rational, or theoretical, science. For it is very doubtful whether such descriptive disciplines as the social studies and psychology have developed peculiar methods of reasoning that are significant in a basic sense. No disrespect is here intended, nor disparagement of the fruitfulness of research in these fields. But their present state is more like the condition of geometry in pre-Pythagorean times, when the three-four-five rule (the forerunner of the Pythagorean theorem) was widely known and used by the Egyptians, though a *proof* of it was lacking. Every science, in its early natural-history stage, proceeds by *correlating* facts, and search for pattern exhausts itself in classification. Only when an element of cogency or necessity is introduced into the body of knowledge, as always occurs when the knowledge is subjected to logical manipulation, does it partake of the character of rational science. And it may be said that our culture has the outstanding property of striving to convert all experience into rational scientific knowledge. This is the precise meaning of our initial assertion.

The suggestion that science has generated or fashioned our culture is no part of my thesis. In endeavoring to gain understanding of an organic development like the history of our civilization, or the growth of living things, it is often dangerous to isolate causal factors; in fact it is often impossible to discriminate between cause and effect. What the thesis does expose is the usefulness of an elucidation of scientific method as a condition for understanding our culture. We therefore proceed with a brief exposition of the methodology of all rational science, suggesting

that it will shed light on the cultural environment in which that methodology was born.

3. The content of experience when caught in its immediacy is flux; it is heterogeneous, colorful, ever changing and it defies classification. The person in the Western milieu has to be reminded of this. Only by conscious effort can he subdue the consequences of his training and become aware of it. The adult who has outgrown the stage of make-believe, for whom the fairy tale has lost its appeal, succumbs to his cultural heritage so completely that he no longer regards fairy tales as descriptive of primitive sensory and emotional experience. He has lost the ability to wonder at the habit of taking the tree in his yard for granted when he is not looking at it. He has in fact yielded to the stabilizing rule which reifies certain phases of his fleeting consciousness. For a true appraisal of the method of science it is important to recognize this situation.

Sense data are never permanent, cleanly defined or sharply separated into entities. They do not permit application of logic, arithmetic or any sort of rational principle. To apply rules, the scientist—and everyone in our culture is a scientist at this stage of perception—passes by means of a constructive act, usually performed without clear awareness, from crude sense experience to the contemplation of ideal structures called external objects. On a higher plane, he resorts to physical entities of a more abstract nature but always endowed with a degree of fixity which the original data did not possess. It is in terms of these and not sense perceptions that he reasons; to them he applies laws and theories; from them he builds his physical world.

Emphasis upon the transition from sensory apprehension to the general entities here in question, together with a clear recognition of the role played by the latter in our understanding of the world, is not merely of epistemological importance. Modern physical theory requires the assumption that laws and theories do not refer solely to sense data as an essential basis for comprehension. Quantum mechanics, for example, is nonsense unless the distinction between sensory experience and physical systems is carefully made and respected. If any doubt arises, the reader should try to confine his attention to the sensory aspects of a piece of matter while crushing it, dividing it more and more finely, and see if he ever arrives at what the physicist means by his imperceptible electrons and their uncertainties. In such instances the difference between *observables* with their chaotic behavior and *physical states* with their regularities becomes the *sine qua non* of comprehension. Its acceptance is also the condition under which scientific experience at all other stages becomes meaningful.

The coordination of theoretical elements[2] with sensory data is not an arbitrary undertaking. Whatever compels the percipient to associate with the complex of sensations brown, hard, rectangular, and so forth, the conception "desk," is not a capricious urge, but a regulatory device of great generality in our culture, though whether it be a necessary constituent of all perception may well be questioned. For present purposes it suffices to state that there are fixed rules of correspondence, fixed in the sense of general acceptance and temporal invariability, which regulate the transition from the fleeting world of sensation to the stable world of public concern.

Not all philosophers will acknowledge the genuineness of this transition; many prefer to regard the public world as also given in sensation. In the early days of science this stand may have been possible, but it is no longer defensible. For in modern physics the very rules of correspondence between data and "constructs" have become matters of serious attention; no longer do scientists argue about their presence, which alone would suffice to force the distinction, but their specific *contents* have taken on increasing importance. For out of the constructs is formed the public world, and this, which is the world described by theory, the world in which predictions are possible, has become exceedingly complex. In the face of this complexity, correspondences with the world of sense are no longer left to instinctive apprehension, are no longer obvious in their immediate givenness;

[2] There appears to be no generally satisfactory name for these entities. I have formerly called them "constructs" of explanation, but have incurred objections on the grounds that they are not constructed like the symbols of mathematics or like rules for playing a game, but have a definite correlation with sense data. Any term applied to them should, however, suggest clearly that they are neither *concepts* in a generic sense, nor *abstractions* from sensory experience.

they require standardization through suitable definitions, called operational. For instance, when the physicist refers to the temperature appearing in the equations of thermodynamics he does not mean sensory coldness or hotness, but a very precise sort of thing *corresponding* to hotness *via* rules laid down in operational definitions. Certainly, to approach an understanding of scientific method is to grant the operation of these rules; their complete analysis is one of the fascinating problems of metascience. It may be that their action is specific in our culture, or even that such correspondences are absent in others where life does not rise appreciably above the charming but irrational level of sensory perception. What matters in the present context is only the stabilizing effect which they exert on our formulation of science, and through it on our mental environment at large.

4. The fact that certain sense data "suggest" (that is, cause us to pass by the afore-mentioned rules of correspondence to) more theoretical essences endowed with greater permanence and pattern, such as external bodies, molecules or genes, does not in itself constitute the latter as true components of what we call reality. For otherwise hallucinations and optical illusions would have to be accepted into the physical world. There are important additional requirements, of which two large classes will here be mentioned. First of all, the entities constructed as counterparts of sense experiences must have relational properties of a logical and mathematical kind so that they can be combined into theories, that is, formalisms which allow modifications of and substitutions amongst the entities. This feature permits the empirical verification, or validation of the constructed entities because it makes possible not only the passage from an initial set of data to a set of "constructs" by means of the rules of correspondence, but also modification of these constructs by theory and final return to *expected* sense data through the same rules of correspondence. If the expectation always meets the data the "constructs" are said to be valid and the theory is verified.

More interesting than this class of requirements looking toward validation is, at least for the purposes at hand, the second. The reader will be spared much mental effort if introduction to the rather abstract problem now confronted is made through an example. Observable phenomena of heat can be "mapped," as it were, upon two scientific universes: one in which heat is a tenuous form of matter, another in which heat is energy. Both of these "constructs" are valid in the sense of all requirements thus far imposed, for both lead to verifiable and verified consequences. Which of the two universes, then, is to be accepted? Or is it to be supposed that certain experiences require the one, the remainder another? Competing theories often arise in the history of science, and frequently both agree with experiment within limited domains. Empirical procedures do not always discriminate between them, especially when no limit is set upon the degree of conceptual elaboration which a given theory is permitted to attain. Choice is then made by reference to regulatory principles which are not always easy to name. In the present instance, the scientist appeals to maxims of *simplicity*, or *economy of thought*, or *logical elegance*, or *generality of application* to settle the dispute, and the result is that the caloric interpretation has been abandoned. It is this group of regulatory principles that is now being considered, to be sure, without intent of careful analysis.

A complete survey of this field in the light of modern science has not been made. Newtonian physics has been penetrated by lucid investigations of this nature and has led to the epistemologies of the last century, none of which is wholly acceptable today. But the fact remains that metaphysical principles which render theories acceptable are still operative in all branches of rational science, and that science cannot function without them. Among them are certainly the maxims that an ideal theory should ultimately comprise all observable phenomena, that its structure be unique in all applications and that it operate in accordance with a postulate of causality. These vague indications do not exhaust the list; they merely hint at larger technical problems and serve to expose the presence of such problems. What is important for the purposes of our inquiry is not the detailed mode of operation of metascientific principles,

but their effect on the body of science and therefore on culture.

This effect is uniformizing and stabilizing. Illustrations of the tendency toward uniformization are available in all historical instances where rival theories formed a synthetic union, where two apparently contradictory interpretations of the same set of facts became reconciled on a plane of higher generality: Witness the resolution of the .wave-particle dualism in quantum mechanics today. The stabilizing effect is caused in very large measure by the arduous and exacting nature of the principles to be satisfied. A theory is not discarded lightly for it is difficult to put a better one in its place. Thence arises, in any given age, a relative permanence in the views of the world, but not an absolute permanence such as might exist if metaphysical principles were not coupled with procedures of validation, or if there were no principles at all. This, again, is a peculiarity of our culture. Sometimes, the conviction of permanence is carried too far. The psychological force which drives many a scientist toward discovery is proclaimed to be the desire for truth, and he himself may firmly believe that he has found *the* final interpretation of his result. But history and methodology both deny this claim.

The principles—here called metaphysical, metascientific, or methodological—which regulate the functioning of scientific theories and whose closer scrutiny is avoided in this article, have themselves an amazing degree of permanence. They appear to change far more slowly than theories, forming in a sense the mental climate in which theories arise. They are akin to the categories of old. The desire to make them absolutely permanent, to anchor them upon the soil of rational necessity, runs through and is highly characteristic of Western philosophy. In fact, if I were to name one thinker who presented the gist of Western culture in its purest form I would choose Kant. Not only is the *Critique of Pure Reason* typical of the adjustment which our civilization makes to successful scientific progress (in this case, Newton's), but the general acceptance of his attempt to locate the categories in the very mechanism for understanding, where no one could easily eradicate them, bespeaks the indoctrination originating in the desire for stable pattern which our culture had undergone.

Among the principles described there is one that may be called the leitmotiv of Western science; it presents all the facts already summarized in clearest outline and accounts for most that is characteristic in our atittude toward the world. In it scientific method announces its credo and exposes its metaphysics to clearest view. It is the belief in the convergence of scientific researches, the belief that all experience can be subsumed under general nonconflicting theories obeying requirements like those sketched in preceding paragraphs. Sprung from a vague notion of the uniformity of nature, the idea worked itself through several philosophic formulations; it is a common factor in such divergent views as rationalism and ordinary realism. Refusal to believe in miracles is *the* sign of cultural enlightenment in our midst.

Regarded without preconception, this metaphysical thesis is a strange postulate, based entirely on the success of science to date. It represents a vast extrapolation upon a limited amount of knowledge and casts the shadows of our present selves on infinite horizons. To be sure, not every scientist holds this view, for the positive results of science can never substantiate it. Whenever rival theories emerge, there is danger of an essential bifurcation, for it might conceivably happen that certain phenomena in a given field are explicable only on one theory, while the rest require another. Three decades ago physicists were almost resigned to this contingency, because light exhibited the contradictory aspects of particles and waves. They were rescued from the dilemma by fortunate discoveries for which at the time there was no certain expectation. Yet the belief in the convergence of scientific explanation is one of the fundamental tenets of Western culture, and I am convinced that its repudiation would alter radically the general structure of our thought.

Whether this change would be a loss, or indeed a disaster, cannot be decided on the basis of an analytic inquiry like the present and will for that reason not be argued. At all events it is worth knowing under what circumstances it may be expected. Our period is one in which causes tending toward abolishment of the axiom of con-

vergence are abundant; they are rising to dominance within powerful positivistic doctrines and are strengthened in the popular mind by unprincipled emphasis on the Scientific Approach, no matter how incoherent or misunderstood. The perspective of history shows clearly that a tendency to ignore methodological bases appears when science has made significant advances and the problem of synthesis is difficult. Thought is then inclined to take the easier course and renounce synthesis. This situation exists today. It is also true that genius, including scientific genius, is rarely bound by conscious reflection to the methodological principles of the past, though of course the cultural bond is always present and is usually recognized after a discovery has been assimilated. Today the probability that assimilation may not take place is considerable.

It may be well to summarize our thoughts so far. A cursory survey of our intellectual life shows a progressive development of rational patterns, a tendency toward uniform and stable concepts and toward norms. We seized upon these as distinguishing traits of our culture and tried to find their origin. It was discovered within the hidden preconceptions that lie at the basis of scientific procedure. Analysis of this procedure brought to light a number of methodological attitudes or principles, among them rules of correlation between our sense world and our public or theoretical world, as well as logical requirements to be imposed on concepts. All of these were seen to have the effect of stabilizing science, and all of them, by being tacitly adopted, mold our lives and create our cultural pattern.

5. Thus far our aim has been to reduce what appear to be the outstanding characteristics of Western culture to relatively few fundamental convictions, and it was shown how these convictions operate in the field that allows them the greatest range of development, namely, science. If the result is on the whole accepted, then we face a peculiar paradox: Western culture, though having produced a distinctive rational pattern, has not provided a basis for ethics. This is superficially apparent in the wide tolerance in regard to what constitutes ethical behavior, but becomes impressively clear when we inquire what our ethics should be if they had developed consistently along with rational views of the world. Only a brief indication will be attempted.

Viewed logically, every system of explanation is a deductive apparatus that starts with postulates, derives theorems and finally tests specific propositions in the realm of data. A suitable example is the physicist's "understanding" of why bodies fall. The postulate is Newton's law of universal gravitation; from it the laws of free fall, or the laws of planetary motion, or the variation of gravitational force on the earth's surface, may be derived by mathematical processes; specific predictions of these theorems are then checked against observation. The postulate itself is never proved in a strict logical sense; it is reflexively validated by confirming its consequences. This is now known to be true even of the most evident of scientific presuppositions, such as the axioms of geometry and arithmetic, since instances are readily found in which they do not hold but require generalization.

There is nothing in the nature of things, or in human nature, that renders this scheme inapplicable to ethics. Consistently, the ethics of Western culture should be a postulational discipline in which a basic code is adopted, developed through its formal consequences and put into practice. The matter of validation, to be sure, raises difficult problems of its own, different in kind from perceptible truth and falsity in the factual realm. But it is hardly correct to say that these difficulties have prevented ethics from crystallizing into a form cognate with the rational structures of our culture. The cause which stunts the growth of ethics in our sphere lies in an irrational insistence upon *a priori* evidence for the very postulates on which a code is founded, a demand never made in science. It is as though we were unwilling to accept the law of universal gravitation, despite its effectiveness as a formal mode of explanation, unless it came to us accredited with unalterable rational necessity or divine sanction. This cultural schism further manifests itself in an imaginary contrast between the domains of "facts" and of "values," the former of which we have been able to handle while the latter proved elusive. To deal with values, artefacts are used which appear rather oriental in the selectivity and specificity of their action, and which bear no resemblance to the uniformity of the methodological principles regulating science.

In concluding this section, I find it difficult to look away from the urgent problem of our time. Does the foregoing analysis add or remove weight from the pessimistic contention that the history of Western culture makes the achievement of world government improbable? To me it seems that the preponderant traits of this culture lend no plausibility to this assertion; that its uniformizing, stabilizing motive force tends in the direction of world unity. But those aspects in which Western culture has not yet come into its own give cause for apprehension.

In Part II, a short analysis of ethics from this point of view will be attempted.

II.

1. A scientist writing on ethics is, and should be, under suspicion. Hence the following remarks have come as the result of conquering serious hesitations arising from the reflection that formalized ethics is not a field with which I have a wide acquaintance. No assurance can thus be given that what is said is at all original. Only a deep concern for Western culture, and a firm conviction of the compatibility of ethics and science, prompt me to argue about matters in which experts may dispute my competence. However, lest this be construed as superficial modesty, I add that I regard the distance between ethics and science as no greater than that between ethics and politics or between ethics and history.

There is a curious circumstance, almost without relevance to the basic problems at issue, which should be noted briefly. Many people believe that the most effective code of ethics is that of the scientist. To be sure, it operates in a very limited domain and almost exhausts itself in what is called *scientific honesty*; it rarely improves the social qualities of those who profess it except in relation to other scientists. But within its narrow range it is remarkably successful. Study of this code will not be pursued here, but is recommended to psychologists interested in ethics. The significant point for present purposes is in its manner of enforcement, in the reason for its effectiveness; scientific honesty is not practiced because of religious beliefs, nor through fear of punishment or hope for reward, nor because of love for fellow scientists. It is adopted because it is part and parcel of the scientific profession; one accepts it without question as one accepts the postulates of arithmetic. Being a scientist implies being scientifically honest.

The fundamental question to be considered is this: Can the method of science, in particular of theoretical science, be applied in ethics? If so, and if scientific methodology is an important characteristic of Western culture, as the main part of this paper affirms, then it would seem consistent to study this application.

There is a vast difference between this question and the apparently similar one: Can science, that is, the factual content of science, be applied to *generate* an ethical code? Confusion of these is disastrous even if common, for it leads to a myopic vision of the whole of human activity. Ethics has a subject matter distinct from that of science. Its advocates are right in insisting that a positive decision which confronts the moral individual is *sui generis*, that science has nothing to say about whether it is right to kill. Only when the decision has been formulated can science become effective in bringing about or averting what man as an ethical person has decreed. Science is ethically neutral, and he who denies this elementary recognition has merely forgotten *when* his moral choice was made. Usually he has become so accustomed to a specific ethical standard, such as hedonism, that he is not conscious of his allegiance and regards it merely, but falsely, as a scientific attitude. The ethical "right and wrong" is different from the scientific "right and wrong" and cannot be developed out of the latter. It is therefore improper to speak loosely of a "scientific code of ethics."

Yet it is equally unwise for the romanticist to go off the deep end at this point and proclaim that ethics is the field of responsible choice and of freedom, while science effects nothing more than a description of the material universe. For science, too, has responsible decisions to make, as the preceding analysis has shown. But its decisions have other purposes and are judged by other criteria. The fact remains, therefore, that the pre-empirical situation in science, its need for choosing methodological principles for guidance, is not below comparison with the pre-empirical state of ethics. Hence the answer to the first

question—Can the method of theoretical science be applied in ethics?—may well be affirmative.

2. To gain further evidence, we recall the essential features of rational science. It starts with *postulates*, that is, general propositions which are accepted as bases for inquiry. These postulates are logically independent of prior fact; they are not inductive generalizations of experience. In their selection, esthetic considerations of elegance, simplicity, and symmetry play as large a role as attention to observable facts, for when a postulate is stated it is frequently uncertain what facts it may imply. Excellent illustrations of these remarks will be found in Maxwell's equations or in the restricted theory of relativity. With respect to the latter it may truly be said that even if facts were to contradict its consequences, physicists would be loath to relinquish it because of the sheer beauty of its postulate of invariance (of physical laws in different intertial systems).

When a scientific postulate has been formulated, it is regarded as *true* for the purposes of subsequent analysis. That no valid or even tentatively valid conclusions may be drawn from it unless its integrity is rigorously respected is a commonplace. The feeling for consistency acquired through a peculiarly authoritarian course of training, a training that brooks no doubt regarding matters of logic, prevents the scientist from using the postulate where it is convenient and ignoring it otherwise in the process of drawing inferences from it. (Nor is this a triviality, for on the basis of *fact* the scientist has just as much reason to doubt that $1+1=2$ as he has for doubting that the human race abhors murder.) The inferences obtained through such integral deduction are usually specific theorems that have something to say about scientific experience.

The first two stages, postulation and deduction, are finally followed by verification. If what the theorems say about experience is found to be true on observation, sufficiently often and under a sufficiently broad variety of circumstances, deductive procedures and postulates are said to form a valid, or true, theory. Thus the logical state of affairs is very simple: the postulates are reflexively validated as premises of propositions which are true. They can never be *proved* in a deductive sense, since there is no assurance that other postulates may not yield equally true propositions, nor are they inductive generalizations of sensory experience.

The process of verification, however, is not as simple as it looks and is often misinterpreted. The nonscientist, and sometimes the scientist, is likely to regard it as a comparison of what is *predicted* with what *is*, and what *is* need not be subjected to further scrutiny, according to this uncritical view. The saying goes that one is dealing here with a realm of indubitable fact which is either correctly or incorrectly described by theory. Astronomers predict the appearance of a spot of light at a definite place in the sky at a certain time; their theory is correct if the spot appears. But what if the seen spot were a mirage? Hallucinations are not predicted by physical science and yet they occur. In fact, there is a great deal in the field of perception that science makes no pretense of predicting. It is customary to divide sensory experience into two categories: *bona fide* perceptions and illusions. The latter are just as vivid, spontaneous, memorable and convincing as the former; indeed, they cannot be separated from them by any criteria involving sense experience alone. Yet physical science does not deal with them directly: it rejects them on the grounds that they are incompatible with its accepted theoretical formalism. Strangely enough, science is selective in its method of verification; *the effect of its formal structure determines to some degree the manner in which it will expose itself to tests.*

Even aside from this there is the difficulty of knowing whether a given observation corroborates or contradicts the results of calculation, since there is never pin-point coincidence between the two. The usual way out of this misfortune is to take a great number of observations, to regard them as a probability aggregate and then to set in motion the machinery of the probability calculus. All of this is far from obvious and simple; its justification has embraced some of the most technical reasoning of modern logic. What matters here is that the process of scientific verification is a very complex one, made possible by constitutive maxims requiring assent in many of its phases; that it far transcends in elaboration

the simple-minded look-and-see operation for which it is popularly mistaken.

The astonishing fact is that scientists never doubt the propriety and essential correctness of their complicated procedures. Why this should be is probably for the psychologist and the anthropologist to say. I believe it to be caused by thoroughly effective conditioning by Western culture in the sense of my initial analysis, in particular to an attitude that permits no doubt with respect to logical axioms and yet demands divine sanction for ethical norms. Perhaps the astonishing qualities of this fact fade away when one contemplates the diverse methods by which we teach (or fail to teach) arithmetic on the one hand, ethics on the other, in elementary schools. If a child says $2+2=3$ we tell him: no, this simply does not happen. If he lies we tell him a story showing that lying is quite possible but leads to unpleasant consequences.

3. We now turn to ethics. To be scientific in the sense here proposed, ethics need not be a mechanical discipline concerned solely with efficiencies or with anthropological findings. It must conform to the three stages of scientific methodology: postulation, deduction of specific consequences and verification. Each of these will take a form suitable to the subject matter of ethics, namely human behavior, and may well differ in detail from its scientific counterpart. But the logical structure should be the same. Thus, it should not be required that the postulates be generalizations from anthropological experience nor that they possess *a priori* evidence. And it is equally unscientific to demand verification through agreement with all human behavior. But let us first consider the three stages one after another.

The stage of postulation presents relatively minor difficulties of conception; it involves the formulation of a code of ethics. To be sure, the *practical* difficulties besetting this act are at present enormous, and they are likely to remain great so long as the postulational, or scientific view of ethics, such as that here sponsored, is not generally accepted. These difficulties, while clearly recognized, are not the object of the present discussion. They will be lessened, however, when the purely postulational or, in the strict sense of the word, *hypothetical* character of the ethical code is recognized. The status of this code should be the same as that of the axioms of arithmetic, or of Euclidean geometry, no more sacred and no less compelling. Especially should it be noted that the code requires no prior motivation in supernatural beliefs. Hence the amalgamation of ethics with doctrinal historical religions violates a most essential part of our thesis; and I share the conviction of many that this amalgamation has been a major hindrance in the ethical development of our culture, as well as others. Perhaps this creative act of postulation, wherein man dedicates himself to principles, has much that is akin to religious experience; but if this is the case, then the relation between science and religion is equally close.

The next stage involves the explication of the codified material for use. In science this is largely an ivory-tower affair; in ethics it is a matter for action, action consistent with the chosen code. Failure to be consistent is in one field regarded as a *mistake*, in the other as a *vice*. One wonders whether this distinction is not unfortunate, whether it is not the result of an indoctrination which fails to appreciate the similarity of the premises in the two situations. All one can say is that possibilities of identifying vice with mistake through more enlightened education have certainly not been explored completely.

The problem of verification in ethics requires more extended comment. Basically, verification is a procedure whereby the validity of the ethical code can be made evident, or can be denied. This is a crucial point, for rejection of postulational ethics is usually grounded on the assertion that such procedures do not exist. True, they do not exist if verification is understood to mean agreement with immediate and indiscriminate anthropological observation, for then the results of an ethical culture would have to be compared trivially with themselves. But with that simple understanding, verification does not work in science, either, as we have attempted to show.

Just what it should be, or will be when postulational ethics is practiced, is not for a scientist to say. But there are several interesting possibilities among which ethics as a going enterprise can choose. First, there is the simple but powerful criterion of group survival, which operates even in the present chaotic situation, whether this be

recognized or not. Certainly, if a code of ethics leads to the extinction of those who practice it, that code will automatically disappear.

It is easy, however, to pass beyond such primitive proposals. The suggestion made by some anthropologists that ethics, to be regarded as successful, should produce a society in conformity with typical human behavior, is usually rejected because it raises the *is* to an *ought*. Perhaps this is dismissing it too lightly. For the emphasis should here be on the word *typical*. Clearly, if ethical postulates are checked against their results in a society resulting from the postulates, validation becomes a rubber-stamping procedure. That society may, however, be atypical, and comparison should perhaps be made with the essence of what is common to all human societies. The vulnerability of this position is entirely clear to me, for I am vaguely conscious of the possibility that typical human behavior may not be definable. But the decision on this point cannot be made by the offhand statement that ethics molds society and hence cannot be tested against it; it must wait upon a clear empirical verdict as to the existence of constancies in aggregate human behavior. Weight may be added to this admonition by the remark that in the subject of dynamics the formulation of Aristotle, which held sway for many centuries, made typical behavior of moving bodies appear impossible. Only when Galilei seized upon acceleration as the pivotal element in the description of motions did constancies emerge and simplicity result.

The list of possibilities for verification of ethical systems can be extended. There are many variations of the theme involving the greatest happiness for the greatest number of people. Perhaps the term freedom can be defined sufficiently accurately to yield a means for validation. Nor is the concern for physical health and well-being to be excluded from consideration.

Perhaps this variety of opportunities is bewildering; perhaps it is so confusing as to preclude action. Historically, disagreement with regard to methods of verification has indeed been a retarding factor in the development of codified ethics. In view of this, the question may be asked: is it necessary that there be absolute clarity on this point at the very outset? There was no such clarity in science at the beginning of its history. Before Copernicus a theory that failed to prove the earth to be flat was likely to be rejected. Different methods of empirical validation have interplayed, and even now a precise analysis of the meaning of scientific confirmation is beset with problems. As natural science developed, it gradually evolved acceptable standards of validity. Is it too much to hope that a similar process will take place in ethics when this discipline is properly launched? All that can be shown at present is that standards are in fact available and that such hope is not a logical absurdity.

What Shall We Say about Airplanes?

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"WHAT things shall be said of you, horses riding the sky?" asked Stephen Vincent Benét, and his question echoes in the staccato tread of "65 horses" high above the campus. *The sky these horses ride can be made safe for our students by applications of first-year college physics.* This is the first and most important thing to be said of them. The present writer says it as a physics teacher with about 1700 hr as a pilot, flying in 43 states, flat and mountainous, cold and hot, all these hours with no accidents, though with plenty of experiences that might have developed into accidents, had it not been for a taste for the pastime of applying first-year college physics.

Perhaps even high-school physics may suffice, if it is supplemented with a course in high-school aeronautics. Nothing is said here about this, because it is outside the writer's field. The term "college physics" is qualified here with "first-year," because the principles of physics that a pilot needs are all in the first-year course.

Applications are emphasized because these principles are so general, as every physics teacher knows, that chances and reasons for applying them are often overlooked.

In this fact lie our opportunity and our duty. The applications needed most urgently by a pilot are excellent illustrations of these principles. They are no better, perhaps, than illustrations previously used, except that they are especially timely. Automobile engines have got so reliable that few people ever look under the hood now, or care what is there. The airplane's "horses" are almost as healthy, and their medical advisers are specially licensed, so the pilot does not have to learn many things directly about them; but certain basic problems of the sky they ride, and how they ride it, can replace some of our older illustrations of principles in first-year physics, with great interest and profit to everyone concerned.

To Fly, or Not to Fly

Illustrating the Bernoulli principle we have the facts behind the most important practical question of all: how to be perfectly sure the ship will keep on flying. Too many ships, even now, suddenly fall and crash; and every crash of this type is preventable with a little physics. Very little, in fact, if this little is properly applied; somewhat more, but still not too much, if one most overcome certain popular misconceptions.

More specifically, the main point is that if the "air speed," or speed of the ship relative to the air, is reduced continuously, at a certain critical value called the "stalling speed" the ship "stalls" —"quits flying" in any reasonable sense of the word flying—and dives toward the ground. Qualifying this rule are two more. One of these is that the stalling speed is proportional to the square root of the load on the wings, whether this load is pure weight or a resultant of weight and centrifugal force (the latter appearing so real, as applied to the pilot's body, that it is not worth while to deny him the use of its name). The other qualifying rule is that, if a ship has stalled, and the pilot makes any attempt to check the dive before enough speed is gained to carry the weight plus the centrifugal load of a "pull-up," this attempt makes a bad matter worse. That is, it is bad or worse if near the ground; up high, it is a harmless amusement. These rules are simple. The last of them may be hard to obey under stress; but why should the stress ever arise? Why not just keep plenty of speed whenever near the ground? Here is where physics comes in.

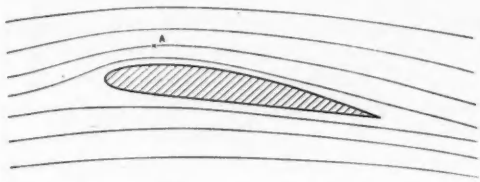

FIG. 1. Stream lines near a wing at a typical angle of attack.

First, of course, any pilot with a reasonable amount of scientific curiosity wants to know why a ship makes this sharp distinction between flying and not flying. He is quite ready to believe that she holds herself up by pushing the air down; also that this downward thrust depends in the obvious way on the "angle of attack," that is, the angle made by the wing chord with the line of flight. This makes him ready to understand that since the wings must support the weight of the airplane, if it is to fly straight and level, any reduction of speed must be compensated by an increase in angle of attack. He is also fairly ready to believe that at typical speeds the flow of air is streamlined, much as shown in Fig. 1.[1]

With this figure or any equivalent he is ready to believe that the speed of the air, relative to the wing, is greater at such a point as A than at a point far out in front, because of suction by the space over the trailing edge of the wing. So he is ready for us to use the deficiency of pressure at A as an illustration of the Bernoulli principle. This illustration becomes even more impressive when he is told that this deficiency of pressure near A accounts for considerably more

[1] The figures in this paper are from the 1941 edition of the *Civil pilot training manual* (Civil Aeronautics Administration, Superintendent of Documents, Washington, D. C.); they were inserted in the *Manual* by the present author, and more details on this and several other illustrations of principles of physics in this paper can be found in the *Manual*. Incidentally, the upward inclination of the streamlines ahead of the wing shown in Fig. 1 really occurs in level flight, as found by the writer in careful tests just after World War I (*J. Frank. Institute*, May 1920). It is caused by indirect action of air going down, behind the wing.

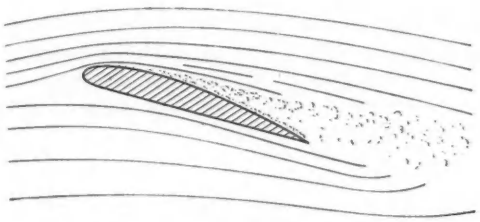

FIG. 2. Stream lines and eddies near a stalled wing.

of the lift than the excess of pressure under the wing—often about twice as much.

Moreover, the restriction in the Bernoulli principle to steady motion takes on a real meaning when the student's attention is called to the fact that a man on the ground, watching the airplane go by and estimating speeds, relative to himself, of the air at points such as A, would get quite contrary results but would have to admit that from his viewpoint the motion is highly unsteady. Still further light is thrown on the principle by first comparing the pressure at A with that at a point far in front, on the same streamline, and then with the pressure at a point far above. In the former of these cases, if the air along a thin tube of flow is divided into a line of blocks, each block is acquiring speed because of a resultant force due to pressure differences; in the latter case, with a column of blocks, the resultant force on each block is downward, to make the block move in a curve. The fact that both methods of finding the pressure at A are valid can be used in contrast to cases of rotational motion—such as the motion of water in a glass jar standing on a steadily rotating plate—to illustrate the restriction of the second method of analysis to irrotational motion, and to illustrate the approximate occurrence of such motion in air that has not been directly subjected to appreciable forces of viscous friction.[2]

Some of these ideas may be a bit farther along than we usually go with Bernoulli in first-year physics, but they can be reached easily with the motive power of "65 horses."

Returning now to the question of why a ship stalls, it must be noted that the suction behind the top of the wing, which draws the air over the top, also tries to draw air up around the trailing edge. It would succeed in doing so if air had no viscosity, and then the wing would have no lift. With real air, the sharper the trailing edge is, the less there will be of this deleterious effect. This is why all wings have sharp trailing edges. Even so, as the speed is reduced and the angle of attack is increased, there comes a limit to the air's patience. Then the air breaks into flow around the trailing edge. This results in a stall, with a mass of eddies[3] (Fig. 2).

In a stall, the air flowing above the wing no longer feels the suction of a big space to be filled, so it does not flow so fast; and it no longer has to turn down so much into such a space, so its streamlines lose much of their curvature. By either of the analyses just outlined, a large part of the deficiency of pressure above the leading half of the wing disappears. This change has two results: (i) a loss of lift, and (ii) a rearward shift in the center of effort of the lift. So the ship, being almost human anyway, seems to have absent-mindedly stepped off the end of a sidewalk. She misses an expected lift on her leading foot, and goes down nose-first—making a reverent bow to Bernoulli.

Keeping Your Speed

This bow does not mean good-bye to Bernoulli—unless the ship is too near the ground. We must not overlook $\frac{1}{2}\rho v^2$, where ρ is air density and v is speed, a factor that has many consequences. One of these, already noted, is the proportionality of the stalling speed to the square root of the load carried by the wing. A similar proportionality holds, of course, for the speed at which the angle of attack takes any specified

[2] Irrotational motion seems like a difficult concept for first-year physics students. One may therefore simply call it motion of air which has not been in any place where there is viscous friction, and let it go at that. (Indirect effects of friction, such as the effect of trailing edge friction on the motion at point A, do not count.) Or one may consider a hypothetical cruciform snowflake (with due apologies to crystallography) and consider the rotational effects of the point-to-point variations of velocity, including its direction, on this flake. Irrotational motion is then motion of air in which this flake will not rotate.

[3] These eddies can be seen in models with streams of smoke in the air, or even better in a full-scale biplane in flight, with bits of tape tied on the struts at various heights. The writer, having previously heard of the latter method, studied them carefully in World War I biplanes and found the upper boundaries of the masses of eddies remarkably definite. If a stall is mild (as when the speed has been reduced slowly), the mass of eddies is thin; if the stall is violent (as after nosing up 30 or 40° with power off), it is thick.

value. That is, it holds, provided all pressure differences involved are too small to make pronounced changes in the density. This proviso is well satisfied by the pressure differences due to any speeds given by "65 horses" or by most of the teams of horses used in private flying.

Nevertheless, changes in density can occur on account of two other variables: altitude and temperature. To obtain any specified angle of attack, these must be compensated by changes of pace by the horses. To a good approximation, the engine speed must be increased by 1 percent for every 500 ft of altitude, by another 1 percent for every 10°F rise of temperature, and by another 1 percent for a drop of 0.6 in. in the sea-level height of the barometer. These rules hold remarkably well for altitudes up to 10,000 ft with reasonable temperatures, or near sea level from −40° to +140°F. They are important, too. A hot-weather trip to Reno can be a serious matter.

Fortunately, Bernoulli is our guardian angel even there. The air-speed indicator also depends on him. It is built like an aneroid barometer, except that it has no spring to keep its box from collapsing. Instead, the box contains air, connected to the outside through a Pitot tube, whose open end faces forward, out in the wind. Here there is an excess pressure $\frac{1}{2}\rho v^2$, where v is the relative speed well in front of the end of the tube. The outside of the box is given "static pressure" by connecting the case of the instrument to a "static head," a tube placed near and parallel to the Pitot tube, but with a plug in its end and little holes in its sides. A good air-speed indicator is calibrated to read correctly at sea level, 15°C, with the barometer at 29.92 in.-of-mercury.

Thus, in circling Reno Airport before landing, if you will keep the air-speed indicator reading the same as in circling your home airport, regardless of temperature or of a low barometer, your ship will "feel at home." *Then so can you.*

Centrifugal Loads

Assuming that a good pilot will always follow the rule just stated, how safe is he, either at Reno or at home, against stalls due to centrifugal overloads? Such overloads can occur either in a quick "pull-up"—a turn upward in a vertical plane—or in a sharp turn in a horizontal plane, or in a combination of these maneuvers.

The theory of pull-ups is just what any physics teacher would expect it to be, even without any aeronautical experience, and it makes excellent illustrative material on centripetal acceleration. The only question that might trouble the teacher is one of appropriate numerical values for the variables. A tendency of everyone is to overestimate the slopes of lines of sight. A 10° glide, which is fairly steep for private flying, at least without flaps, is likely to be called 20°; and a real 20° glide would be called by harsh names. A 5° climb is steep for a light airplane, and 10° cannot be held without deceleration.

Moreover, all student pilots are told to relax, be smooth, take it easy, and so on; and all good pilots follow this rule. An airplane takes a surprisingly long time in making any change of speed, because longitudinal forces on it are usually small fractions of its weight; so the pilot never has to hurry any maneuver. Thus it would be rough flying to pull up from a 10° glide to a 5° climb in less than about 3 sec. These figures yield an angular velocity of about 0.1 rad/sec. If the linear velocity is 96 ft/sec, the centripetal acceleration is then only about $0.3g$. The pilot therefore feels as if he weighed about 1.3 times his actual weight, and the wings must carry 1.3 times the load they carry in straight flight. The ratio of any such temporary load to normal is called the "load factor."

For a load factor of 1.3, the stalling speed is increased in the ratio $1.3^{\frac{1}{2}}$, or 1.14; for example, from 40 to 45 mi/hr. If the air speed is 65 mi/hr, a gentle pull-up such as this presents no danger. Indeed, to raise the stalling speed from 40 to 65 mi/hr would require a pull-up that would take the ship from a 10° glide to a 5° climb in only 0.5 sec—rough flying indeed. Moreover, this pull-up would result in a load factor of 2.6, which feels very unpleasant, even up where it is safe; so most of us would not be tempted to do it anyway. In short, violent pull-ups near the ground are dangerous, but not tempting; on the other hand, with any air speed at least 50 percent above stalling speed, gentle pull-ups are safe.

Incidentally, if a physicist wants to measure some load factors, just for curiosity, he can make the necessary instrument easily. One, made by

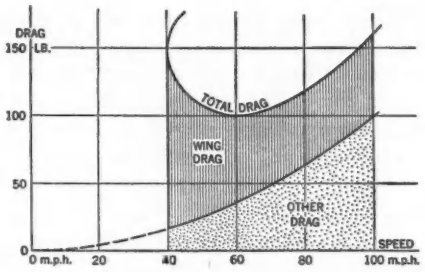

FIG. 3. Drag *versus* speed, for a typical light airplane, assumed to weigh 1000 lb, to glide 10-to-1 at 60 mi/hr and to stall at 40 mi/hr.

one of the writer's students, consists of a $\frac{3}{4}$-in. glass tube 9 in. long, containing a brass cylinder 1 in. long, hung on a light steel spring, with anhydrous isopropyl alcohol as a damping fluid. The cylinder moves nearly an inch per unit of load factor, so the scale is easy to read.

Regarding the 50-percent margin of speed previously mentioned, it must be regretfully noted that on many of the popular, light, high-wing airplanes, the Pitot-static head is mounted under the leading edge of the wing, where the relative speed of the air is abnormally low, especially at high angles of attack. Such a mounting exaggerates the difference between stalling and cruising speeds (not to the deep regret of the manufacturer's salesmen); therefore the speed margin as read this way should be more like 70 percent than 50.

Fortunately, this safe speed for gentle pull-ups is also about the speed at which most of these ships climb best, for reasons to be outlined later; so it is a familiar speed, and the pilot has no occasion to fly any slower except in taking off from rough ground or in landing. Then his wheels are within a few feet of the ground while the speed is lower, so a stall would not be fatal; but it might damage the airplane, so these are definitely bad occasions for quick pull-ups.

The theory of turns of constant altitude, like that of pull-ups, is just what any physics teacher would expect, and it illustrates both centripetal acceleration and the vector addition of forces. In a well-made turn the ship is banked so that the wing span is perpendicular to the required lift, which is naturally the equilibrant of weight and centrifugal force. (*Lift* is defined as the component of aerodynamic force, exclusive of propeller thrust, perpendicular to the line of flight.) This bank does the same for the pilot as for the ship, so he "rides with the ship"—lets his body be banked with it—and recognizes the correct angle of bank by a part of what he calls "feel." If the bank is not steep enough, the ship "skids" like a car on a slippery road, causing pressure on one side of the fuselage, and he feels his body tending to be tipped off the seat by centrifugal force, which is real to him. On the other hand, if the bank is too steep, the ship "slips" toward the center of the turn, and he tends to be tipped by gravity. If it is just right, he feels right.

This kind of "feel" is important with most ships now flying, because it is the basis on which the pilot coordinates his rudder and ailerons. Its acquisition can be greatly accelerated by use of a ball-type bank indicator, with which the pilot can tell when his coordination is correct. Its importance is waning fast, however, with an increase in the number of ships of the two-control type invented by Fred E. Weick (Ercoupe and others) in which both ailerons and rudder are operated by one of the controls (by turning the control wheel), and all turns are banked well enough for all practical purposes, regardless of the pilot's "feel."

In this connection it should be noted that a "spin" is a condition in which one wing is stalled, while the other keeps on flying; and a spin can be started only by stalling in an improperly banked turn, usually with a bad skid. This cannot be done with a ship of the Weick type; therefore these ships are certified by the Civil Aeronautics Administration as "characteristically incapable of spinning."

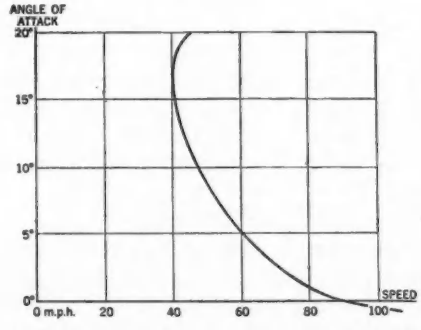

FIG. 4. Angle of attack *versus* speed, for the airplane of Fig. 3.

With the proper bank, how sharp a turn is safe, with a 65-mi/hr air speed in a ship stalling normally at 40 mi/hr? The load factor 1.3, for that gentle pull-up, is sec 40°. So a "gentle turn" (defined by the CAA as up to 30°) is likewise safe here; but the pilot may be advised against making this turn and the pull-up at the same time. "Medium turns" (30° to 50°) range from safe to somewhat dangerous if made at this speed near the ground, especially in rough air. "Steep turns" (50° to 70°) under these conditions range from somewhat dangerous to certainly fatal. In short, the rule for safety against centrifugal loads is simply: when more than 2 or 3 ft off the ground, and less than several hundred feet, *always keep at least as high an air speed as in "best climb," plan all maneuvers to have plenty of room, and make only gentle pull-ups or gentle turns; then you are safe.*

One more important fact about centrifugal loads is that the Civil Air Regulations require that in any part of a ship stressed by load, the yield point must exceed the stress occurring when the load factor is 4. This theoretically permits turns at constant altitudes requiring banks up to 75°; but it does not guarantee safety beyond a load factor of 4, and the pilot must make allowance for errors in his estimates! If he wants to bank steeper, for the fun of it, he must not do it at constant altitude, unless with such a steep slip that he is far from the aforedescribed coordination. A better way to bank steeper in a low-powered ship is to fly a curve lying in a surface inclined in a manner obvious to a physicist.

Gliding, Climbing and Cruising

The theory of glides, considered from the viewpoint of illustration of principles in first-year physics lectures, belongs with the old, familiar inclined plane, as one might expect—and somehow an airplane arouses much more interest than does another block with a different friction coefficient.

Since glide angles in private flying are usually small, the lift required for keeping the glide straight is practically equal to the weight of the ship. This simplifies the theory. Strictly, the drag (defined as aerodynamic force component backward along the flight path) is a function of both air speed and lift; but with the lift practically constant the drag may be treated as a function of only one variable, the air speed.

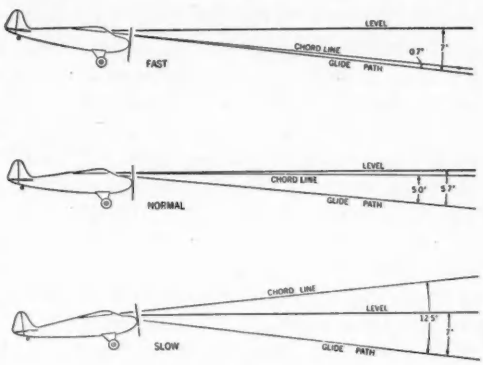

FIG. 5. Two 7° glides and the "normal glide," or "best glide," for the airplane of Fig. 3. The "slow" glide is dangerous if near the ground.

In Fig. 3[4] it is obvious, at least to a physicist, that the slope of a straight, unaccelerated glide in still air is a function of its speed. Further, it is obvious that the "best glide," also called the "normal glide," which carries the ship farthest in still air with a given loss of altitude, is a glide at 60 mi/hr; also that the glide angle at this speed is practically 0.100 rad, or 5.7°.

At other speeds, the glide angle is evidently a single-valued function of the speed, but the speed is a double-valued function of the glide angle. For example, a steady glide at 7° would require a drag of 1000 sin7°, or 122 lb, and this is the drag at either 82.5 or 43.5 mi/hr. So the question arises in what attitude to hold the ship with the controls, so as to attain this glide angle and one or the other of these speeds. This involves the relation of angle of attack to speed, which is shown (again idealizing to some round numbers) in Fig. 4.

From this graph the angles of attack for these speeds may be found. Then, combining these angles with the given angle of glide, the required attitudes of the airplane may be found; they are shown in Fig. 5.

The faster of these two 7° glides may be

[4] The reasons for the changes in "wing drag" and "other drag" shown in this figure are discussed in reference 1. Similar discussions can be found in many books, such as E. P. Warner, *Airplane design* (McGraw-Hill, ed. 2, 1936).

called "very firm." That is, if an "air pocket" (local downdraft) drops the line of glide from 7° to 8°, but the pilot uses his elevators to keep the ship in the same attitude (wing chord pointing down 6.3°), the angle of attack in the still air beyond the pocket is changed from 0.7° to 1.7°. While the lift is not exactly proportional to this angle, and most wings have a finite lift at zero angle, such a change as this makes a big change of lift. This brings the glide path back quickly to 7°; and, since there is very little change of drag, the speed is practically unaffected.[5]

The slower 7° glide, on the other hand, is much less firm. In it, if an air pocket drops the glide path to 8°, the change in angle of attack is from 12.5° to 13.5°. This changes the lift less than in the ratio 13.5/12.5, and very much less than 1° changed it at the higher speed; so the return to 7° is relatively slow. Meanwhile, the unduly large angle of attack increases the danger of stalling if any other roughness in the air happens to change the relative wind unfavorably. Slow glides, near the ground, are not only infirm, but dangerous.

This comparison in firmness may be beyond the range a teacher wants to cover in the lectures, but it is worth knowing about because students who have observed it in flight so often wonder why it occurs. Likewise it is well to know the consequences of raising the nose 1° in either glide and thereafter holding it in the new attitude. For several seconds, of course, the glide path also is raised; then it settles back as speed is lost. The faster glide never settles all the way to 7°, but only to something between that and the 6° to which it has been raised. The slower glide, on the contrary, settles beyond 7°. This happens because at 7°, with the new attitude, the drag in the slower glide is larger than before the pull-up, so the speed is still decreasing. The question arises, therefore, whether the increase in the forward component of weight, as the glide path settles, ever catches up with the increase in drag. With this ship under the postulated initial conditions, it does—at 7.6° and 41.5 mi/hr, if the air is smooth enough to permit such a speed with a ship that stalls at 40 mi/hr.

[5] See, for example, curves of coefficients of lift and drag *versus* angle of attack, found in many books, such as reference 4, Fig. 42.

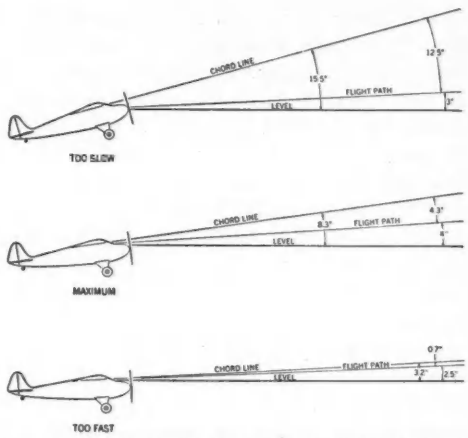

Fig. 6. Three climbs for the airplane of Fig. 3. The "too slow" climb is dangerous if made near the ground.

A second gentle 1° pull-up stays up for an even shorter time and settles to 8.1°, 40.3 mi/hr; and a third, if it can be made at all, soon settles into a stall. The temporary successes of such gentle pull-ups have deceived many badly trained pilots approaching airports into "trying to stretch a slow glide" instead of using the throttle. Such attempts have caused many fatalities.

The theory of climbs is a direct extension of the theory of glides, made by adding another force, the propeller thrust. This force really is a function of two variables, engine speed and air speed, and its direction is slightly above or below the line of flight except at the angle of attack which points the propeller shaft exactly along this line. In a first approximation, however, the component of propeller thrust normal to the flight path may be neglected, since it is very small in comparison with the lift. The changes of thrust with speed, at the speeds used in climbing, also are scarcely important enough to be worth the effort needed for including them in a first-year course. On this basis, some typical climbs are shown in Fig. 6.

With these facts in mind it is obvious that the speed giving the steepest climb is very near the best for gliding; actually, it is a very few miles per hour slower. The "maximum climb," or "best climb," however, is the one in which the most altitude is gained per unit time, and this is very few miles per hour faster than the best glide. For long climbs, it is still better to use a

higher air speed, so as to keep the engine cool, and not try to gain altitude so fast.*

Cruising, either level or in a gentle climb or let-down, is naturally done at least as fast as the speed for minimum drag. There would be no object, even of saving fuel, in going any slower. So the safety rule—keep at least the speed for best climb whenever near the ground—involves nothing that a pilot would not naturally do for other reasons—except perhaps, that he must be very sure to do it!

For quiet, smooth cruising in the sort of ship under discussion, a convenient speed is about 70 mi/hr. To see what her "65 horses" are doing in level cruising is a good illustration of the simple formula for power calculation. Taking the drag from Fig. 3 as 107 lb, one finds the power to be 20 hp. This is the power that is left after deducting 20 or 30 percent for the imperfect efficiency of the propeller, and a bit more for the extra drag due to impact of the slipstream on the fuselage. Altogether, therefore, in cruising at 70 mi/hr, the "65 horses" are working at only about half their full power. Just as well. It is nice to know, especially over a stretch of thirsty-looking country, that you are not driving them hard.

Taking on a passenger, say a 210-pounder, including his baggage, in this ship the 1000-lb total weight is increased to 1210 lb, or in the ratio 1.1^2. Therefore, recalling the Bernoulli v^2, it is evident that if the ship is gliding in the same attitude as in any one of the glides shown in Fig. 5, and at the same glide angle, all forces will balance again with the speed 1.1 times what it formerly was.

In cruising, likewise, *if* you increase the air speed and also the engine speed, each in the ratio 1.1, all forces will balance again at the same angle of attack. You may do this, or not, somewhat as you please.

My own ship, for example, has characteristics very nearly as given by these figures, and an increase from 70 mi/hr alone to 77 with such a passenger, near sea level at about 60°F, requires an increase in the engine speed from 2000 to 2200 rev/min. The latter is a bit high for cruising, and the corresponding speed at an altitude of several thousand feet would be entirely too high. So I compromise: 2100 rev/min near sea level, with about 73 mi/hr. Conversely, however, if I want to go very easy on the horses (as in crossing the Georgia swamps under a low overcast!) or if I am just up enjoying the scenery with no urgent business, I drop the speed near sea level without the passenger to about 73/1.1 = 66 mi/hr with a quiet 1900 rev/min.

For "best climb," the engine speed, of course, is the maximum which you see fit to make it for the length of time involved. Taking off out of a short airport, this means full throttle; once clear of the trees it means a bit less; but in either case it is the same, with or without the passenger. Therefore, when taking the passenger, one increases the air speed for best climb (say from 62 to 68 mi/hr) so as to keep it in the same relation to the speed giving minimum drag, but one must accept a much lower angle of climb and resist the temptation to try for more.

The "Feel for Speed"

The "feel for turns" has been described here, but not yet the "feel for speed," although this part of "feel" is one of the most common topics for comment by many flight instructors. It is indeed complex.

A large part of the feel for speed is a sense of that firmness of the flight path which was discussed under the theory of glides. In cruising or climbing, the same degrees of firmness occur at the same speeds as in gliding. At high speeds in rough air, the ship "rides hard"; at low speeds, she is "mushy." Judging one's air speed by this part of the feel for speed is conventionally, if not elegantly, called "flying by the seat of your pants."

The rest of the feel for speed is a sense of the firmness of the control stick or wheel. The higher the speed, the stronger is the aerodynamic force on any control surface when the control is deflected at a given angle; and these forces can be felt indirectly on the stick.

By the feel for speed, altogether, in reasonably rough air a skilful pilot can estimate his air speed fairly well. Not quite so exactly as by looking at a good air-speed indicator, of course; and indeed, since feel has no graduated scale,

* *Note added in proof:* It is assumed here that the propeller has a fixed pitch, as in most light airplanes, and that this pitch is the usual compromise between the desiderata for climbing and cruising. A more elaborate airplane, with a propeller of variable pitch, climbs best with a low pitch and therefore at an appreciably lower air speed for best climb. At such a speed, turns and pull-ups must be even gentler than at the speed for best climb with a propeller of fixed pitch.

if he says how many miles per hour he feels, he is saying what he remembers reading on the air-speed indicator at other times with the same feel.

With the air density reduced because of high altitudes or temperatures, speeding up the engine to get the same air-speed reading as at home gives the pilot also the same feel as at home. This is why, as previously noted, under these conditions the ship will feel at home, and so can he.

For example, suppose you fly on a summer day from San Francisco to Reno, that is, from sea level at 60°F to 4400 ft at 90°F. Your true air speed is raised 9 percent for altitude and 3 percent more for temperature; so you would like to speed up the engine by the same percentages. If the ship is mine, with that 210-lb passenger and an indicated air speed of 73 mi/hr over each airport, this would mean a change from 2100 rev/min at San Francisco to 2350 at Reno. The latter is too fast, since there is a red line on the tachometer at 2300, so you go only to 2300 and let the indicated air speed drop back by about 2 mi/hr. But if you used 2100 rev/min at Reno (though you could never have got over the Sierra Nevada without about 2300) this would be like only 1875 at San Francisco. A drop of engine speed or its equivalent, like this, would cause a drop of air speed by a greater percentage (because of the increase in angle of attack and in drag coefficient); so this change would bring the indicated air speed down notably below the 66 mi/hr that gives minimum drag with the passenger.

With special care you might "get away with it"; but the most widely quoted last words in flying are, "I'm pretty sure I can make it." The worst of this use of 2100 rev/min would be a loss of subconscious cues. In this respect flying is much like riding a bicycle. When the front wheel of the bicycle hits a loose pebble you don't consciously think what to do about it; you just do it. Rough air is treated in the same way, especially when the pilot's conscious attention is on such questions as which runway to use, how far that other fellow is going to turn, and so forth. To keep his reactions to the air good, he needs the right feel. A loss of 2 mi/hr is inconsequential, but a less to below best climbing speed is serious. In short, there is more than sentiment in saying "Giddap" to the horses, so that the ship and you will feel at home.

On the other hand, there are real dangers in relying exclusively on feel. If the air is completely smooth—as it often is, almost anywhere, early in the morning—the part of feel depending on roughness of riding simply does not exist. Moreover, the other part, depending on firmness of the control stick, fails to supply any subconscious cues, because if you have no occasion to move the stick for a long time, there are no reminders of whether it is firm or limp. Of course a pilot *can* wiggle the stick often, just to get the feel of it, but usually he doesn't. Even if he does, he gets only one of the two parts of the feel for speed, and that part not in the subconscious way that is the chief advantage in feel. In smooth air, therefore, it is very dangerously easy, if relying too much on feel, to stall the ship and crash.

Another danger, inherent in the subjective character of feel, is the ease with which it can be overridden by other influences. For example, in approaching a mountain range, not steeply enough to clear it, the skyline of the range should appear to rise slowly, but by a common, persistent illusion this rise is not perceived. This illusion can easily lead a pilot into the climb shown in Fig. 6 as "too steep," so it has caused crashes. Another bad influence, more common and much worse, is wishful thinking, especially under stress, as in climbing out of a windy little airport among hills. This can override feel much too easily.

But neither smooth air nor human weakness can pervert a good air-speed indicator. Obviously, the moral of all this is: *Glance at that air-speed indicator, once in a while, as a check.* Glance at it; *don't stare at it.* Remember that the ship's speed never changes fast, because the longitudinal forces are small; so with any speed as high as that of best climb there is no use in any cat-watching-a-dog attitude. Moreover, if the air is clear enough so see anything far ahead, so as to be able to hold the ship at a reasonably constant attitude, and if the throttle is not changed, any longitudinal unbalances due to bumps are extremely small—a man sleeping in a berth in a prewar airliner never got his head

bumped, even in very rough air; so these glances at the air-speed indicator don't even have to be very frequent.

Simple, this is. Incredibly simple, considering how many lives could be saved by it, every year. Then why does any pilot neglect it? Here it is difficult indeed for the writer, after long experience, many conferences and careful study of the accident records in the prewar *Air Facts* and *Air Commerce Bulletin*, to maintain a dispassionate attitude. The reason is, that many flight instructors—not so many as before the war, but still too many—tell their students, "Never look at that thing! It's always wrong!" or "It's busted! Never was any good," and so on.

Well, is it wrong? Is it "busted"? And if so, how? One way it has sometimes been "busted," to the writer's direct knowledge, has been by the flight instructor's closing his mouth over the Pitot tube and blowing hard, to make sure it would be "busted." Because of what this attitude signifies, some manufacturers of "training planes" have been careless about the quality of instruments they install. Fortunately this carelessness is disappearing. Nowadays any reputable manufacturer provides instruments that don't break of their own accord.

Even some of the good manufacturers, however, still calibrate their instruments incorrectly, in the interest of sales. This is a nuisance. Navigation with such an instrument amounts to having "a built-in head wind." But any good pilot can take the ship up to a safe height, and find by trial the air-speed reading at which the ship stalls. Then with a watch and the altimeter he can find also the air-speed reading for best climb. From here on, all is simple. He does not need to worry at all about the real air speeds, so long as he remembers and uses the best-climb reading.

What, then, is there against the air-speed indicator? One interesting bit of physics, and some psychology. Physics comes in because of the ship's slowness in changing her speed. This is popularly mistaken for lag in the instrument. If a student notices that it reads lower than he wants, it is natural for him to lower the nose to pick up speed. Eventually he gets the speed he wants; but if he knows no physics he has been disappointed at not getting the speed sooner, and has kept on lowering the nose. So the speed keeps on increasing. Next he starts to reduce it by raising the nose, and so on. This procedure builds up an annoying pitching oscillation, with a period of 20 or 30 sec and an amplitude limited only by the instructor's impatience or fear of stalling. If the instructor knows no more physics than the student, one should not blame him too much for blaming the air-speed indicator.

The remedy, however, is simple. Tell the student the facts; and tell him that if he wants more speed he should lower the nose a little, by guess at how much, then hold it it fixed by the horizon and *wait about 10 sec* to see how much more speed he gets. This 10 sec is a very long time, and the first guess may not be just right, but a second guess and another 10 sec will fix it. A little physics does wonders.

Boners

A student's answer to a test question: "Dopper's [sic] principle established the fact that the appearance of two stars seem to us as if a pebble were dropped in a pond or when a train crosses a grade crossing and the bell is ringing—the passengers as they ride hear it and then faintly—the stars move and they may be still but actually have moved."

[*Contributed by Robert S. Shaw, College of the City of New York.*]

"The essential difference between frequency modulation and amplitude modulation is in the fact that the electronic waves are varied in intensity of frequency as they are transmitted, while in amplitude modulation they are varied in length (or in the interim of the time interval) of the frequency. It was soon discovered that the upper portion of the spectrum heretofore exclusively utilized by short wave transmission was favorable to frequency modulation performance. After a long period of experimentation in the 40- to 45-megacycle band, the Federal Communications Commission decided to lift frequency modulation licensing to the 88- to 108-megacycle band which approaches the frequency of light."—From an article in *Higher Education* **3**, 3 (Dec. 1, 1946) entitled "Colleges Take up FM Radio Licenses," by the Chief, Educational Uses of Radio, U. S. Office of Education.

[*Contributed by George H. Burnham, Norwich University.*]

Biophysics*

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BEFORE attempting to enlist physics as a tool for large-scale investigation of the fundamental problems of biology, it is necessary to examine closely the concept of biophysics and the contribution that physics can make to this field. Since the major field is biology, the subject should most properly be called *physical biology*. Biophysics, however, has such an associative advantage that it will probably be retained as a name, although it is a less exact description. Broadly, it encompasses the application of physical concepts to biological problems. With its partner, biochemistry, and with the considerable assistance of physical chemistry, it seeks to describe submicrobiological phenomena in a quantitative fashion.

The deeper one goes in understanding the fundamental nature of biological processes, the less useful are the distinctions between biophysics and biochemistry. In the limit, the exact definition of biophysics becomes purely a matter of convenience which does not provide any real guidance in showing how physics can be applied to biology.

Immediately obvious are two ways in which physics can react upon biology: by introduction of physical technics and of physical concepts. Since biology deals with living organisms, a province completely foreign to physics, these technics and ideas must be applied to biology with the greatest care; the distinction between the two fields is as important as the likeness.

The most popular example of physical technic is probably the use of isotopes as tracers. But there are in addition many more, such as the use of high-energy radiation, the electron microscope and the manifold new applications of spectroscopy and electronics. The demonstrated successes of these technics account for the fact that many people in the fields both of biology and of physics have come to regard physics as a source of instruments, and the physicist, in his biological work, as a glorified technician. One cannot gainsay the achievements, but one can be sure that instrumentation alone is not in itself the real solution to the integration of physics and biology. For the present, the artificial support of instrumentation alone will yield results, as does any artificial insemination. In the long run, however, instrumentation alone can lead only to a sterile end. At best it is applied physics, and cannot be expected to lure competent physicists into the biological field.

To the physicist, at least, the exciting contribution that physics can make is conceptual. The physicist believes that his traditional ways of attacking a problem are powerful and effective, and hence that biological systems should yield to such treatment. He has found further that the close combination of theoretical and experimental physics has proved extremely fruitful and he, therefore, believes that this effective combination can be extrapolated to the much more complicated biological systems. Exact and quantitative observation has served him well. If he can validly extend his concepts to biological phenomena, important results should arise.

The gross phenomena of anatomy and physiology are now well understood. The microscope provided a powerful tool to enable men to comprehend fine distinctions. Future great discoveries lie hidden in the submicroscopic region, awaiting the development of senses perceptive enough to penetrate further.

The very complexity of biological problems requires the collaboration of many branches of science. Hence, it has stimulated the creation of research groups containing experts in many related fields. The chemist, working in the synthetic organic field, has long been a welcome member of such teams. The physicist is fast becoming one. But in both cases, one cannot

* This manuscript was prepared in the course of duties as technical aide to the Committee on Growth, Division of Medical Sciences, National Research Council. It represents the author's views, modified and expanded by discussion with other workers in the field, particularly Drs. Henry Borsook, Britton Chance, Martin Kamen, Francis Schmitt and S. Spiegelman. It was written before publication of the excellent article by J. R. Loofbourow, *Am. J. Physics* **15**, 21 (1947), and represents a different approach to the subject.—A.K.S.

expect any real stimulus from the inclusion of these members unless they become well trained in biological discipline, so as to be able to examine the problem adequately from a biologically informed point of view. The real strength of a group emerges only when all of the members are co-partners in devising the experiment and evaluating the results.

Group research is required for adequate examination of many of the contemporary problems. For example, the mechanism by which food is transformed into energy and this energy into work in a living organism is still not clear. The use of isotopes and the study of intermediary metabolism have provided some clues to the solution of this basic problem. Research on the mechanics of nerve transmissions has provided others, yet the solution is still distant and the way tortuous. The physics, the kinetics and the physical chemistry of many of these processes remain to be studied in detail, providing probably the most fruitful source of discovery for the next several years.

A considerable body of descriptive information has been amassed in experimental embryology, yet our knowledge of the laws responsible for the development of the egg into the adult is at best meager. It is now necessary to discover and explain the chemical and physical functions of aggregates of matter of microscopic size. The powerful physical technics already applied to problems such as this are not in themselves enough. Further giant strides can be expected from a fresh approach, in which the wisdom of the biologist, the power of a theoretician and the concepts of the physicists are closely welded together.

Education.—The specifications for the physical biologist are much easier to set out than they are to fulfill. A true hybrid must be educated according to a scheme different from the traditional education of either the biologist or the physicst. The physical biologist cannot emerge full grown except from the few institutions where adequate instruction is now available. In the early stages, it will be necessary to create him by teaching physicists biology, and conversely, by teaching biologists physics. A growing nucleus of men trained in this way will soon provide instruction on a large scale in physical biology.

Although the exact curricular details are rightly dependent on the university which provides the instruction, it is still possible to set out the fundamentals. In addition to a basic biological training at the undergraduate level, certain other skills are essential. A major premise of biophysics is that the exact sciences are indivisible from biology; therefore the calculus is indispensable as a useful tool. In physics the student must understand the basic laws of electricity, magnetism, mechanics, sound and light. In addition, as a well-educated scientist, he should have some slight familiarity with quantum mechanics, and atomic and nuclear structure. From the chemist he must learn organic and physical chemistry, with special reference to biological systems. Laboratory experience, so important in establishing the physical concepts, must run through all this training, like a river narrow at its source, growing ever broader.

The philosophic basis of physical biology is an overt acknowledgment of basic scientific unity. A bold course, at the undergraduate level, which treated the study of science historically would go a long way towards making the student understand the basis of this unity and would also prepare his mind for a consciously integrated outlook. Such a course would demand a broad understanding of science and philosophy on the part of the instructor. It offers a challenge which, met successfully, would provide a magnificent stimulus.

Few graduate schools feel that it is necessary to offer formal instruction in the diverse arts of experimentation. These arts, from glass blowing and soldering to the preparation of biological materials, are learned by apprenticeship, and all too often are learned badly. While in research it may not be advantageous or wise to build one's own apparatus, it is certainly necessary to design it. Proper design stems only from experience and knowledge of the materials available. Applied experimental training is particularly necessary in biophysics, where it must be extended to include training in the use of the electron microscope, isotopic tracers and the design of simple electronic instruments.

The graduate school is a place to specialize in one branch of biophysics. Whatever the branch, graduate training is also essential in biochem-

istry, in physical chemistry including thermodynamics and reaction rates and in statistical mechanics. The student must learn biophysical theory, including the laws governing energetics in biological systems. He should be able to discriminate between those theories that can be taken over almost unchanged from chemistry and physics, and those that need to be radically modified in the light of biological conditions.

Professional contact in the graduate school with men proficient in neighboring fields will not only broaden the student's outlook, but will also serve to increase the biophysical interest of those engaged in research on the fringe of biophysics. The biological "feel," so essential to success in this field, can only be learned from people with many years' experience in basic biological problems.

It is clear that thorough training according to this scheme will probably require at least an additional year in graduate school. The discipline is so arduous that thorough training becomes mandatory. Once trained, the physical biologist will be adequately equipped for the intelligent study of some of the most stimulating scientific problems Nature has set.

On a Generalization of the Poiseuille Law

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BLOOD flow in the vascular system is believed to be described in terms of physical laws. Poiseuille[1] set out to discover the relationship of size of tubes and pressure gradient with rate of flow of fluids in capillaries. For his principal investigations his tubes were of glass and the fluid was water. Whole blood, however, does not have a viscosity as constant as that of water; nor do blood vessels have walls as rigid as those of glass.[2] In fact, the unsatisfactory state of the blood-flow problem at the present time accentuates the need for a generalization of the Poiseuille law. This is essential to an understanding of fluid flow in tubes of rubber and other high polymers, and of fluids of variable viscosity. Such a generalization should have numerous other applications.

Theory

In this analysis Newton's fundamental relation,

$$F = -\eta A \mathrm{d}v/\mathrm{d}z, \qquad (1)$$

is applied. In it F is the viscous drag of a moving fluid, having transverse velocity gradient $\mathrm{d}v/\mathrm{d}z$, on a surface of area A. For generality, the coefficient of viscosity η is assumed to depend on velocity and velocity gradient. We search for a relation giving the time-rate of flow Q as a function of applied pressure difference between the ends of a tube, wherein the distension varies along the entire length as a result of decreasing fluid pressure.

For this purpose let the undistended tube have a uniform radius r_0, and suppose that the radius at a distance x from the end of higher pressure is r ($r > r_0$), when a fluid flows in streamlines through the tube. Clearly, the pressure falls off in the direction of flow and, consequently, the tube walls become less distended. Furthermore, the streamlines are not parallel to the tube axis, but converge toward it.

Let yr be the radial distance from the axis at x to a point in the fluid, so that y is a dimensionless parameter ranging in value from zero to unity. The axial velocity v at this point is a function of both x and y. Evidently, $\mathrm{d}z$ is equal to $r\mathrm{d}y$, and the inner surface of a cylindrical shell of length $\mathrm{d}x$ has an area $2\pi r y \mathrm{d}x$; so that the viscous drag g on this surface is

$$g = -2\pi\eta \mathrm{d}x y \mathrm{d}v/\mathrm{d}y. \qquad (2)$$

[1] J. L. M. Poiseuille, "Experimental investigations upon the flow of liquids in tubes of very small diameter," trans. by W. H. Herschel, *Rheological Memoirs* (E. C. Bingham, Easton, Pennsylvania, 1940). For an excellent review of Poiseuille's investigations see also J. F. Herrick, *Am. J. Physics* 10, 33 (1942).

[2] H. D. Green, "Circulation: physical principles," *Medical physics*, O. Glasser, ed. (Year Book Publishers, Chicago, 1944), pp. 212–218.

For the outer surface of the shell, the radius is $r(y+dy)$, and the viscous drag is $g+dg$. Therefore, the resultant drag on the cylindrical shell is

$$G=-dg=2\pi dx d(\eta y dv/dy). \quad (3)$$

In order to maintain fluid flow, a force equal in magnitude to G must be applied between the ends of the cylindrical shell. On the upstream end the applied force is

$$f=2\pi p r^2 y dy, \quad (4)$$

where the pressure p is a function of x only. On the downstream end it is $f-df$. The resultant applied force is

$$F=2\pi y dy d(pr^2), \quad (5)$$

since r is a function of x, and y is not. Finally, on equating F and G, one obtains

$$y d(pr^2)/dx=d(\eta y dv/dy)/dy. \quad (6)$$

This is the differential equation for the flow of fluids with variable viscosity through tubes having nonrigid walls.

On integrating Eq. (6) with respect to y we see that the integration constant must vanish, and we have

$$\tfrac{1}{2} y d(pr^2)/dx=\eta dv/dy. \quad (7)$$

If no slipping of the fluid takes place along the surface of the tube, a second integration yields the equation for the velocity profile, namely,

$$\int_0^v \eta dv=\tfrac{1}{4}(y^2-1)d(pr^2)/dx, \quad (8)$$

and the velocity v' at the center of the tube is given by

$$\int_0^{v'} \eta dv=-\tfrac{1}{4}d(pr^2)/dx. \quad (9)$$

The rate of flow Q through a cross section of the tube is found by integrating the product of velocity and element of area over the entire cross section. In this discussion the element of area is $2\pi r^2 y dy$ and, therefore,

$$Q=2\pi r^2 \int_0^1 v y dy. \quad (10)$$

Substituting from Eqs. (7) and (9) in Eq. (10), we obtain

$$Q=\pi r^2 \int_0^{v'} \eta v dv \Big/ \int_0^{v'} \eta dv. \quad (11)$$

This equation is the generalized form of the Poiseuille law. The ratio of the two integrals yields an average speed for the fluid at the cross section of radius r. Since Q is invariant along the tube under steady conditions of flow, this average speed must be inversely proportional to the square of the radius at any cross section.

For viscosity independent of velocity and velocity gradient, Eq. (11) reduces to

$$Q=-\frac{\pi r^2}{8\eta}\frac{d(pr^2)}{dx}. \quad (12)$$

The case of variable viscosity will not be discussed here.

Applications

Tubes with rigid walls.—For tubes of uniform bore, the radius r is constant, so that the pressure gradient along the tube is also constant. Therefore, Q may be written

$$Q=\pi r^4(p'-p)/8\eta l, \quad (13)$$

where p' and $p(p'>p)$ are the pressures at the ends of the tube of length l. Equation (13) is the Poiseuille formula.

For tubes of nonuniform bore, r is a function of x. For instance, suppose that the tube is conical, so that r is given by $mx+a$; then for steady flow,

$$Q=-(\pi r^3/8\eta)(r dp/dx+2mp), \quad (14)$$

where

$$p=p'(a/r)^2-(8\eta Q/\pi m a)(r-a)/r^2. \quad (15)$$

Tubes with thin Hookian walls.—If Hooke's law is valid for the walls of the tubes, the fluid pressure p and radius r are related by the equation[3]

$$p-p_0=2e_0E(r-r_0)/(2-\sigma)r_0^2, \quad (16)$$

where p_0 is the external pressure; e_0 is the thickness and r_0 is the radius of the wall when p equals p_0; and σ and E are the Poisson ratio and

[3] R. V. Southwell, *Theory of elasticity* (Oxford, ed. 2, 1941), p. 147.

the Young's modulus, respectively, of the wall material. Inserting the value of p from Eq. (16) into Eq. (12) and integrating between radius a at $x=0$, and radius r at $x=x$, one obtains the following relation between r and x:

$$Q=(\pi/8\eta x)\{\tfrac{1}{2}p_0(a^4-r^4)+[2e_0E/(2-\sigma)r_0^2]\times[\tfrac{2}{5}(a^5-r^5)-\tfrac{1}{2}r_0(a^4-r^4)]\}. \quad (17)$$

If one uses the pressure p' at the upstream end of the tube where r is equal to a, then e_0E may be eliminated from Eq. (17), which becomes

$$Q=(\pi/8\eta x)\{\tfrac{1}{2}p_0(a^4-r^4)+[(p'-p)/(a-r)]\times[\tfrac{2}{5}(a^5-r^5)-\tfrac{1}{2}r_0(a^4-r^4)]\}. \quad (18)$$

For a tube with rigid walls and uniform bore, r is equal to a and Eq. (18) reduces to Eq. (13).

Tubes with thin elastomeric walls.—In this case the fluid pressure p and radius r are related by the equation[4]

$$p-p_0=(e_0p_0/2r_0)\{(r_0/r)^{\frac{1}{2}}\times[\mathfrak{L}^{-1}(\beta r/r_0)/\mathfrak{L}^{-1}(\beta)]-(r_0/r)^{\frac{1}{2}}\}, \quad (19)$$

where p_0, e_0 and r_0 have the same meaning as before, and $\beta\equiv 2\pi r_0k_e$. The parameter k_e is inversely proportional to the reciprocal of the maximum molecular chain lengths within the elastomer; it characterizes elastomers just as the Young's modulus characterizes Hookian materials. The functions $\mathfrak{L}^{-1}(\beta)$ and $\mathfrak{L}^{-1}(\beta r/r_0)$ are inverse Langevin functions; for instance, if u_0 represents the inverse function $\mathfrak{L}^{-1}(\beta)$, then β is equal to the Langevin function $(\coth u_0-1/u_0)$. For small values of the inverse Langevin functions—0.5, say—the functions can be replaced by three times their arguments, so that

$$p-p_0=(e_0p_0/2r_0)[(r/r_0)^{\frac{1}{2}}-(r_0/r)^{\frac{1}{2}}]. \quad (20)$$

[4] A. L. King, *J. App. Physics* **17**, 501 (1946).

We substitute for p in Eq. (12) its value from Eq. (20) and integrate between the limits a, the radius at the upstream end, and r at point x along the tube. For tubes with sufficiently thin walls Q then comes out to be

$$Q=(\pi/8\eta x)[(p'a^4-pr^4)-\tfrac{1}{2}p_0(a^4-r^4)]. \quad (21)$$

Again, if the tube is rigid and has a uniform bore, so that $r=a$, Eq. (21) reduces to Eq. (13).

The Poiseuille equation is time tested. For accurate determinations of viscosity coefficients, however, corrections are introduced to take care of nonsteady conditions at the ends of the tube and the kinetic energy of flow.[5] The relation for tubes with Hookian walls, Eq. (18), is more difficult to verify experimentally. Such high pressures would be needed that the term involving p_0 probably could be negligible. The relation for thin-walled elastomeric tubes, Eq. (21), shows that the volume of flow per unit time through a cross section of such a tube is not proportional to the difference in pressure between the ends, even when the external pressure is zero. This result is of significance in the study of blood flow within vessels of the body.

Poiseuille's original investigations were undertaken in order to learn more about blood circulation. The equation which is named after him, however, has been found inadequate in blood-flow analyses. Blood vessels are more like rubber than glass tubes and, therefore, the generalized form of the Poiseuille law, along with the law of elasticity for elastomers,[6] should be used.

[5] G. Barr, *A monograph of viscometry* (Oxford, 1931), pp. 15–35.

[6] A. L. King, *Am. J. Physics* **14**, 28 (1946); see also reference 4.

Meeting of the Philadelphia Section

The Physics Club of Philadelphia—a Section of the American Association of Physics Teachers—met on March 21, 1947, in the Randal Morgan Laboratory of Physics, University of Pennsylvania. The speaker was Dr. H. C. Rentschler, of Westinghouse Electric Company. His subject was "Gas and vapor discharges."

MABEL A. PURDY, *Secretary*

A Geometric Construction for the Location of the Cardinal Points of a Thick Lens

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IN textbooks of geometrical optics the most generally presented methods of determining the cardinal points of a thick lens are algebraic. Formulas are deduced from either the Halley or the Newton conjugate position equation in conformity with an arbitrarily chosen sign convention. By substitution of the appropriate lens constants in the formulas, the required positions may be calculated. Care must be exercised in the interpretation of signs attached to calculated distances. In the account that follows it is shown, as an alternative to the algebraic treatment of the problem, how the positions of principal foci, nodal and unit points and the optical center of a thick lens not necessarily in one medium may be found by geometric construction.

The light being supposed to proceed from left to right, the sign convention used is the familiar one of coordinate geometry by which distances measured from an origin to the right are positive, and to the left, negative. A merit of this convention is that its use involves less mental adjustment and liability to misinterpretation of signs than others in current use. The geometric method is an extension of the Cartesian graphical representation of the Halley and Newton equations and yields a figure in which the relative positions of cardinal points are immediately evident. A complete solution of the problem is afforded thereby. Further, consideration of similar triangles in a figure of construction leads easily, and without direct reference to the conjugate position equations, to the standard thick lens formulas.

The image-forming properties of single refracting spherical surfaces and thin and thick coaxial spherical surfaced lenses are, if the rays concerned are paraxial, expressed algebraically by either the Halley equation, $(f'/l')+(f/l)=1$, or the Newton equation, $xx'=ff'$. Here f and f' are the principal object and image focal lengths, that is, the distances of the principal foci from the self-conjugate pole in the case of a single refracting surface or thin lens, or from conjugate points of unit transverse linear magnification (unit points) in the case of a thick lens or combination of lenses; l and l' are conjugate object and image distances measured from the same origins as are the focal lengths; and x and x' are object and image distances measured from the corresponding principal foci. All the symbols are algebraic, and in calculations appropriate signs

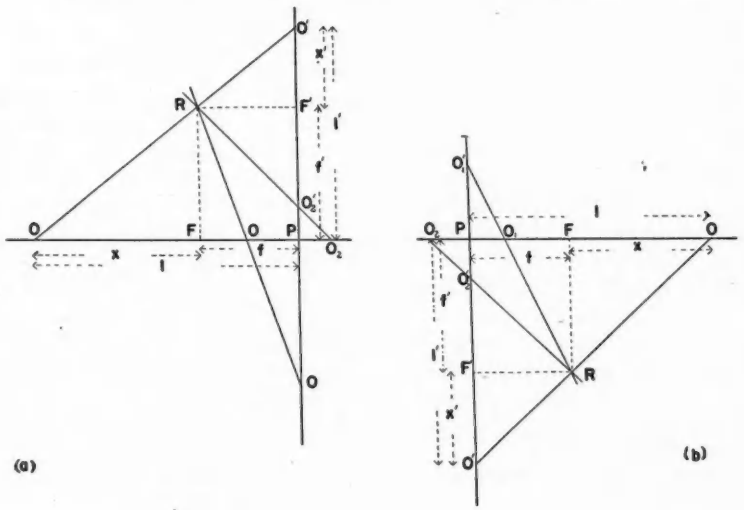

FIG. 1. Cartesian graph representing $(f'/l')+(f/l)=1$, $xx'=ff'$. Origin for f, f', l, l' is P; origin for x is F, and for x' is F'. (a) Refraction by converging lens or surface; f negative, f' positive. (b) Refraction by diverging lens or surface; f positive, f' negative. Straight lines through $R(f, f')$ give by their axial intersections the positions of conjugate points. O_2 in (a), O and O_1 in (b) relate to virtual objects. (The lowest O in part (a) should read O_1'. The O at the intersection between R and the aforementioned O should read O_1.)

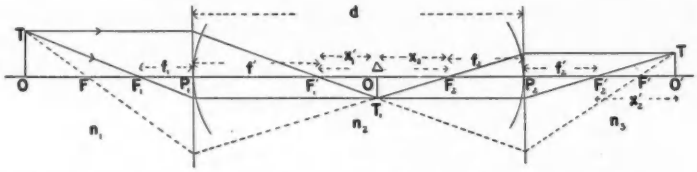

FIG. 2. Ray diagram illustrating the production of an intermediate image O_1T_1 and the final image $O'T'$ of an object OT by a thick lens whose refractive index n_2 is greater than either of the indexes n_1 and n_3 of the mediums on opposite sides of the lens. Broken lines indicate a graphical determination of the principal focal points F and F'. (The T at the right edge should read T'. The f' should read f_1'. The O at the center of the diagram above T_1 should read O_1.)

must be attached to numerical values in accordance with a sign convention.

In the following account the sign convention adopted is the Cartesian system recommended by the Physical Society[1] in which distances along the axis measured from the appropriate origin are positive in the initial direction of progress of the light and negative in the opposite direction. Transverse distances measured at right-angles to the axis are positive upwards and negative downwards. Thus if, as will be assumed throughout this paper, the light proceeds from left to right, the sign rule is identical with that of Cartesian coordinate geometry. No separate rule is required for signs of angles or radii of curvature, the origins for the latter being the poles of the surfaces concerned.

The Cartesian system specified is particularly suitable for the well-known graphical representation of the equations $(f'/l')+(f/l)=1$, $xx'=ff'$ with respect to perpendicular axes of coordinates in which object distances are measured along the horizontal axis and image distances along the vertical axis as shown in Fig. 1, (a) and (b).

For a single refracting surface or thin lens the origin P corresponds to the pole or lens center (coincident unit points). For a thick lens the unit points H and H' are regarded as coincident with P; f and l are measured along the actual lens axis from H, and f' and l' from H'. The focal lengths f and f' are opposite in sign and are unequal if the mediums on the two sides of the lens have different refractive indexes; R is the point whose coordinates are (f, f'). The intercepts PO, PO' on the axes by any straight line through R are the distances l, l', while FO, $F'O'$ are the distances x, x'. A consideration of similar triangles shows that the foregoing equations are completely and consistently represented. The possibility of the object being virtual is provided for since the intercept l may be positive. For example, in Fig. 1(a) the virtual object O_2 has a real image O_2' (l' positive), and in Fig. 1(b) the virtual objects O and O_1 have, respectively, a virtual image O' (l' negative) and a real image O_1' (l' positive). The transverse linear magnification is $-f/x=-PF/FO$. With the appropriate signs attached to the numerical values of PF and FO, an image is erect or inverted according as the magnification is positive or negative.

The equations are applicable without change of form to spherical mirrors. Then f and f' are always equal and have the same sign; apart from this difference the graphical representation is as for a lens.

Determination of the Cardinal Points of a Thick Lens

A thick lens may be regarded as a combination of two coaxial refracting surfaces to each of which the preceding graphical method may be applied. The extension of the method in this manner will be shown to lead to a simple geometric construction for locating the positions of the principal foci, unit points, nodal points and optical center of the lens whether the mediums on the two sides of the lens have the same or different refractive indexes.[2] Further, by con-

[1] *Report on the teaching of geometrical optics* (Cambridge Univ. Press, 1934).

[2] A brief account of the method in the special case of the lens in air has been given by A. S. Percival, who attributes it to R. A. Sampson [*Proc. Optical Convention* II, 274 (1912)]. In this case unit and nodal points coincide in pairs, and it is not shown how, in general, the unit points may be determined. Also, the construction as given is justified by reference to thick lens formulas. In the present treatment the more complete construction will be verified from first principles.

sideration of similar triangles in the resulting diagram, the usual formulas for the relative positions of the cardinal points will be derived—a treatment that appears to be more direct and less liable to error, particularly in respect of signs, than the purely algebraic one.

Consider the ray diagram (Fig. 2) for the formation of the image $O'T'$ of the object OT by a biconvex lens of thickness d whose refractive index n_2 is greater than the indexes n_1 and n_3 of the mediums on opposite sides of the lens. The object and image focal points are F_1 and F_1' for the first surface on which light is incident and F_2 and F_2' for the second surface. Refraction at both surfaces is convergent so that f_1', f_2' are positive. The distance between F_1' and F_2, known as the "optical interval," or "separation," is denoted by Δ; it is always measured from F_1' and so is positive in the diagram. At O_1T_1 is the intermediate image. The determination of the resultant focal points F and F' is indicated by the broken lines.

In what follows it is important to note that F, F_2 are conjugate for the first surface and that F_1', F' are conjugate for the second surface. This is true generally for any lens irrespective of the signs of Δ and of the radii r_1, r_2 of the surfaces.

The Construction

The radii and refractive indexes being given, calculate f_1, f_1', f_2, f_2' from

$$f_1=-n_1r_1/(n_2-n_1), \quad f_1'=n_2r_1/(n_2-n_1),$$
$$f_2=-n_2r_2/(n_3-n_2), \quad f_2'=n_3r_2/(n_3-n_2).$$

Alternatively, the component focal lengths may be determined graphically as follows. Let light proceed from a medium of refractive index n to one of index n' separated by a surface of radius r_s. To find the focal lengths f_s and f_s' draw perpendicular axes, the origin P of which, as regards horizontal distances, corresponds to the pole. Mark the position of the center of curvature C on the horizontal axis. On the vertical axis mark the points M', M such that $PM'=n'$ (positive), $PM=n'-n$ (positive if $n'>n$, negative if $n'<n$). Join CM and produce it to intersect the horizontal line through M' at F. Let a line through M' parallel to CM intersect the horizontal axis at F'. Then $M'F$ and PF' give the focal lengths f_s and f_s', respectively, in magnitude and sign.

In Fig. 3, $n=n_1$, $n'=n_2$, $n_2>n_1$, $r_s=r_1$ (positive). It is seen from similarity of triangles that the equations

$$\frac{n_2-n_1}{r_1}=\frac{n_2}{f_1'}=\frac{-n_1}{f_1} \text{ are satisfied,}$$

f_1 and f_1' being given in magnitude and sign by $M'F$ and PF', respectively.

It may be noted that the foregoing equations give at once $f_1+f_1'=r_1$. Correspondingly, in Fig. 3, $CF'=M'F$, so that once f_1' has been found, f_1' may be deduced immediately without drawing $M'F'$ parallel to MC.

Plot the positions of the surface poles P_1, P_2 and of the focal points F_1', F_2 to scale as on the lens axis (Fig. 4); F_1', F_2 will always be on the horizontal axis irrespective of the signs of f_1' and f_2. Draw vertical axes through P_1 and P_2, thus completing pairs of perpendicular axes with origins P_1, P_2. Plot $R_1(f_1', f_1)$ with respect to P_1, and $P_2(f_2, f_2')$ with respect to P_2, having regard for signs.

Resultant foci F, F'. Join F_2R_1 and produce to intersect the P_1 vertical at F; join $F_1'R_2$ and produce to intersect the P_2 vertical at F'.

Resultant nodal points N, N'. Join R_1R_2 and produce to intersect the P_1, P_2 verticals at N, N'.

Optical center N_0. This is the point of intersection of R_1R_2 and the horizontal axis.

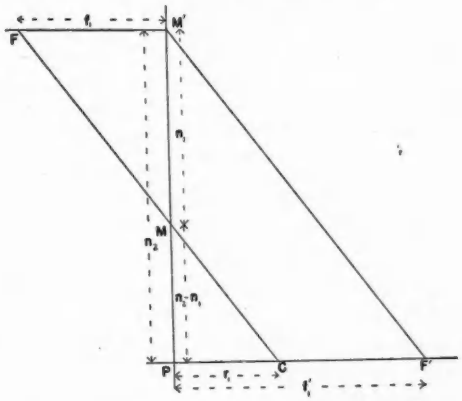

FIG. 3. Construction for determining focal lengths of a single refracting surface of radius r_1 when the medium on one side has an index n_1 and that on the other side has an index n_2.

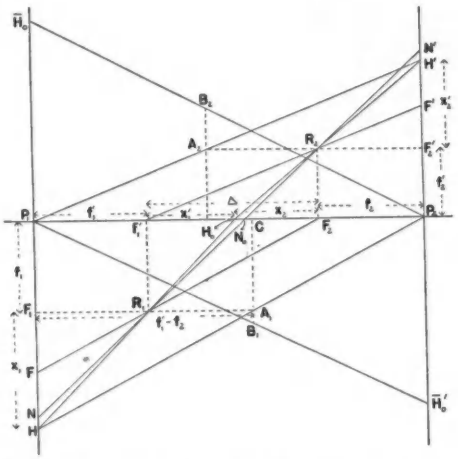

FIG. 4. Construction for determining distances from the poles P_1 and P_2 of the cardinal points F, F', N, N', H, H' and the optical center N_0 of a thick lens.

Resultant unit points H, H′. These points can be determined in any of three ways:

Construction (i). Produce F_1R_1 to the point A_1 such that $F_1A_1=f_1'-f_2$ algebraically; in the present case, $f_1'+f_2$ numerically. If $f_1'-f_2$ were negative, production would be to the left. Join P_2A_1 and produce to intersect the P_1 vertical at H. Similarly, $F_2'R_2A_2[=-(f_1'-f_2)]$ gives P_1A_2H'.

Alternatively, if either H or H' be found as just described, the other unit point is found more directly by joining the known unit point H, say, to R_1 and producing to the horizontal axis to find H_0, the point to which both H and H' are conjugate. Then H_0R_2 produced gives H'.

Construction (ii). Plot the point B_1 whose coordinates with respect to P_1 are $f_1'-f_2$ (horizontal), f_2 (negative in Fig. 4, vertical); P_1B_2 produced intersects the P_2 vertical at $\bar{H}_0'$. Thus the distance of $\bar{H}_0'$ from P_2 is found and, noting the sign, H_0 may be plotted on P_1P_2. The determination of H, H' follows at once as in (i). Alternatively plot B_2, the coordinates of which are $-(f_1'-f_2)$, (f_1') (origin P_2). Join P_2B_2 and produce to intersect the P_1 vertical at $\bar{H}_0$, thus obtaining the distance $P_1\bar{H}_0$.

Construction (iii). Draw P_2H parallel to F_2F to give H; draw P_1H' parallel to $F_1'F'$ to give H'.

The positions of cardinal points on the lens axis may now be marked by means of compasses centered first at P_1, then at P_2. Points on verticals below P_1 or P_2 will appear to the left of the corresponding pole and points above P_1 or P_2 to the right.

If one of the lens surfaces is plane, the corresponding radius of curvature and component focal lengths are severally infinite. The ratio of the focal lengths, however, remains finite and determinate. Thus, if $r_1=\infty$, $f_1'/f_1=-n_2/n_1$, and though the point R_1 (Fig. 4) cannot be plotted it may be considered to be on the line through P_1 inclined to the P_1P_2-axis at an angle $\theta=\cot(-n_2/n_1)$. F_1' is at infinity on the horizontal axis, and F_1 is at infinity on the vertical axis through P_1. F is now determined by the intersection of the P_1-vertical by a line through F_2 inclined at angle θ to P_1P_2, while a horizontal line through R_2 (passing through F_1' at infinity) intersects the P_2-vertical at F'. F' is seen thereby to coincide with F_2', as may have been foreseen since a parallel beam incident first on the plane face of the lens is converged or diverged on refraction at the second surface only. A line through R_2 inclined at angle θ to P_1P_2 passes virtually through R_1 and cuts the horizontal axis at the optical center N_0; its intersections with the P_1, P_2-verticals give the positions of N, N', respectively. H and H' may be found in accordance with construction (iii). A line through P_2 parallel to F_2F (that is, inclined at angle θ to P_1P_2) intersects the P_1-vertical at H, and a line through P_1 parallel to R_2F' (horizontal) cuts the P_2-vertical at H'. Thus H' is at the pole of the curved surface.

The construction is facilitated and rendered more accurate by the use of squared paper. It is simpler in application than would appear from Fig. 4, which illustrates the various alternatives for unit points. Only five lines, apart from the axes, are required in the most general case (see Figs. 6 and 9), and three if the lens is in one medium so that H_0 and N_0 coincide (see Figs. 7 and 8).

Proof of the construction

(1) *Principal foci F, F′.*—As previously noted (Fig. 2), F_2 and F are conjugate for the first surface refraction, as are F_1' and F' for the second. Hence F_2, R_1, F and F_1', R_2, F' are, respectively, collinear.

(2) *Nodal points N, N′.*—The points N, N' are conjugate axial points such that a ray entering the lens directed through N leaves it in a parallel direction through N'. For these points the angular magnification is unity and positive.

In Fig. 5, N_0 is conjugate to N and N'. The angular magnification for N and N_0 is α_0/α

(negative); and for N' and N_0, α'/α_0 (negative). The angular magnification for N and N' is $\alpha'/\alpha=(\alpha'/\alpha_0)(\alpha_0/\alpha)=1$. Hence

$$(P_2N_0/P_2N')(P_1N/P_1N_0)=1,$$

so that the triangles P_1N_0N, P_2N_0N' in Fig. 4 are similar, angles P_1N_0N, P_2N_0N' are equal and N, N_0, N' are collinear.

(3) *Optical center* N_0.—All rays through N and N' pass through N_0. Therefore all rays that are undeviated by the lens pass through N_0.

(4) *Unit points* H, H'.—Since by construction (i), Fig. 4, P_2F_2 and A_1R_1 are equal and parallel, P_2A_1H and F_2R_1F are parallel. Similarly, P_1A_2H' and $F_1'F'$ are parallel. To verify constructions (i) and (iii) it remains to prove that H and H' are unit points.

From Fig. 2 the transverse linear magnification is given by

$$O'T'/OT=(O'T'/O_1T_1)\cdot(O_1T_1/OT)$$
$$=(P_2F_2/F_2O_1)\cdot(F_1'O_1/P_1F_1').$$

If this magnification is unity and positive, O and O' are unit points (H and H') and O_1 is the point conjugate to both and so corresponds to H_0. Thus in Fig. 4, in order that constructions (i) and (iii) should give the positions of unit points, P_2F_2/F_2H_0 should be equal to $P_1F_1'/F_1'H_0$; that is,

$$-f_2/-x_2=f_1'/x_1', \text{ or } (f_1'-f_2)/(x_1'-x_2)=f_1'/x_1'.$$

Now the triangles F_1A_1H, $F_1'F_2R_1$ are similar, and triangles F_1R_1H, $F_1'H_0R_1$ are similar; hence

$$F_1A_1/F_1'F_2=F_1H/F_1'R_1=F_1R_1/F_1'H_0,$$

so

$$(f_1'-f_2)/(x_1'-x_2)=f_1'/x_1',$$

as required.

With regard to construction (ii) we have, from similar triangles $P_1P_2\bar{H}_0'$ and P_1CB_1, $P_2\bar{H}_0'/P_1P_2=CB_1/P_1C$, whence

$$P_2\bar{H}_0'=-f_2(f_1'+x_1'-x_2-f_2)/(f_1'-f_2).$$

As already shown, if H_0 is conjugate to unit points, $f_2/x_2=f_1'/x_1'$. Writing $x_1'=f_1'x_2/f_2$ in the expression for $P_2\bar{H}_0'$, we obtain $P_2\bar{H}_0'=-x_2-f_2$; that is, $P_2\bar{H}_0'=P_2H_0$, and construction (ii) is verified.

It will be shown geometrically that the distances HN and $H'N'$, measured respectively from H and H', equal the algebraic sum of the resultant focal lengths f and f'. If the lens is in one medium these are numerically equal but opposite in sign, so that in this case N, H; N', H'; N_0, H_0 coincide in pairs, and the construction for the nodal points gives also the positions of the unit points.

When F_1' and F_2 coincide the lens is telescopic; F, N, H, $F'N'H'$ are at an infinite distance from it, and the optical center is coincident with F_1', F_2.

The construction is applicable without modification to a system of two coaxial thin lenses whose distance apart is d, when f_1, f_1' and f_2, f_2' denote, respectively, the pairs of focal lengths of the lenses.

Applications

(1) *A thick biconcave lens with water on one side.*—Figure 6 illustrates the construction when $r_1=-5$ cm, $r_2=6$ cm, $n_1=1.33$, $n_2=1.63$, $n_3=1$, $d=2$ cm. The component focal lengths are $f_1=22.2$ cm, $f_1'=-27.2$ cm, $f_2=15.5$ cm, $f_2'=-9.5$ cm, $f_1'-f_2=-42.7$ cm. Method (i) is used to determine H and H'. Measurement gives the following distances: $P_1H=1.0$ cm, $P_2H'=0.4$ cm, $HF=f=7.7$ cm, $H'F'=f'=-5.8$ cm, $HN=H'N'=1.9$ cm, $P_1N_0=4.1$ cm.

(2) *Ramsden eyepiece.*—This eyepiece (Fig. 7) consists of two identical plano-convex lenses placed with their curved faces turned towards each other at a distance equal to two-thirds the focal length of each lens; thus $f_1'=f_2'$ (positive), $d=2f_1'/3$. The cardinal points are symmetrically placed, the focal points being outside the system and the unit points between the lenses; $P_1H=3d/4$, $HF=-9d/8$, $P_1F=-3d/8$.

(3) *Huygens eyepiece.*—The two lenses of this

FIG. 5. Nodal points N, N', optical center N_0. An incident ray through N emerges in a parallel direction through N', and both rays are conjugate to the ray that intersects the lens axis at N_0. The angular magnification (α'/α) is unity and positive.

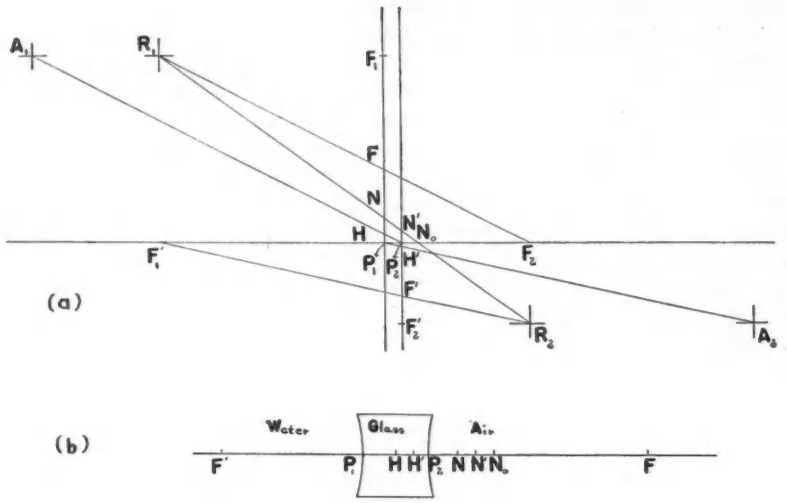

FIG. 6. Thick biconcave lens with water on one side, air on the other. (a) graphical construction, (b) axial positions of F, F', N, N', H, H', N_0 relative to lens poles P_1, P_2.

eyepiece (Fig. 8) are also plano-convex, placed with their curved faces towards the incident light. The focal length of the field-lens is thrice that of the eye-lens, and the distance between them is twice the eye-lens focal length. Thus $f_1'=3f_2'$, $d=2f_2'$, and F_1' and F_2' coincide. The first principal focal point F is between the lenses, whereas in the Ramsden eyepiece it is in front of the field-lens. It is because of this difference that the latter is the more suitable for use in telescopes incorporating cross wires or scales for purposes of measurement. Here $P_1F=3d/4$, $P_1H=3d/2$, $P_2F'=d/4$, $P_2H'=-\frac{1}{2}d$.

Derivation of Formulas for Calculation of Positions of Unit Points, Principal Focal Points, Nodal Points and Optical Center of a Thick Lens

The required formulas will be obtained by consideration of similar triangles in Fig. 9, which is a simplification of the construction diagram.

P_1H, P_2H', measured from P_1, P_2.—Denote the algebraic values of these distances by h, h', respectively, so that in the figure relating to the lens previously specified, $h=-P_1H$, $h'=P_2H'$, that is, $P_1H=-h$, $P_2H'=h'$. This kind of substitution of algebraic symbols will be used throughout, and the formulas obtained will be quite general and applicable to any lens.

In the similar triangles P_1HP_2, $F_1'R_1F_2$,

$$P_1H/P_1P_2=F_1'R_1/F_1'F_2,$$
$$-h/d=-f_1/\Delta,$$
$$h=f_1d/\Delta. \qquad (1)$$

In the similar triangles $P_2H'P_1$, $F_2R_2F_1'$,

$$P_2H'/P_1P_2=F_2R_2/F_1'F_2,$$
$$h'/d=f_2'/\Delta,$$
$$h'=f_2'd/\Delta. \qquad (1')$$

HF, $H'F'$ (the principal focal lengths f, f'), measured from H, H'.—From similarity of tri-

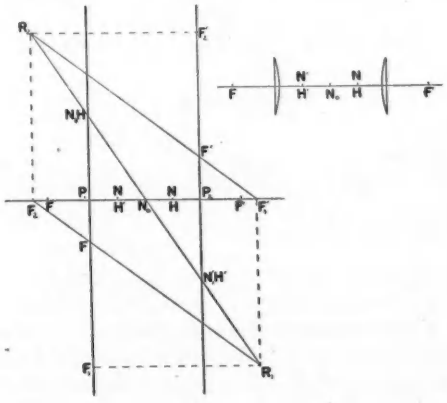

FIG. 7. Ramsden eyepiece, a symmetrical system. The N above the H' should read N'.

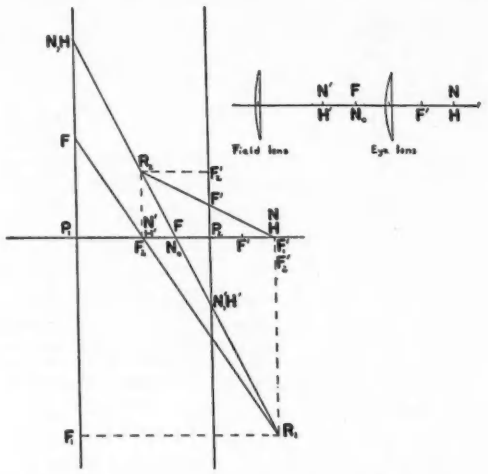

FIG. 8. Huygens eyepice, a nonsymmetrical system.

angles, P_1FF_2, P_1HP_2, $F_1'R_1F_2$,

$$HF/P_2F_2=R_1F_1'/F_2F_1',$$
$$f/-f_2=-f_1/\Delta,$$
$$f=f_1f_2/\Delta. \qquad (2)$$

From similarity of triangles, $P_2F'F_1'$, $P_2H'P_1$, $F_2R_2F_1'$,

$$H'F'/P_1F_1'=R_2F_2/F_2F_1',$$
$$-f'/f_1'=f_2'/\Delta,$$
$$f'=-f_1'f_2'/\Delta. \qquad (2')$$

HN, $H'N'$ (denoted by p, p'), measured from H, H'.—From similarity of triangles, NFR_1, $R_2F_2R_1$ and FF_1R_1, $R_1F_1'F_2$,

$$NF/R_2F_2=F_1R_1/F_2F_1',$$
$$NF=f_1'f_2'/\Delta=-f',$$
$$HN=HF-NF,$$
$$p=f-(-f')=f+f'. \qquad (3)$$

From similarity of triangles, $N'F'R_2$, $R_1F_1'R_2$ and $F'F_2'R_2$, $R_2F_2F_1'$,

$$N'F'/R_1F_1'=F_2'R_2/F_2F_1',$$
$$N'F'=f_1f_2/\Delta=f,$$
$$H'N'=F'N'-F'H',$$
$$p'=f-(-f')=f+f'. \qquad (3')$$

P_1N_0, P_2N_0, distances of the optical center N_0 (c and c') from the poles of the lens, measured from P_1, P_2.—Denote the distance $F_1'N_0$ by e (algebraically, measured from F_1'). In the similar triangles $F_1'N_0R_1$, $F_2N_0R_2$,

$$F_1'N_0/F_1'R_1=F_2N_0/F_2R_2,$$
$$e/-f_1=(\Delta-e)/f_2'=\Delta/(f_2'-f_1),$$
$$e=-f_1\Delta/(f_2'-f_1). \qquad (4a)$$

Also,

$$P_1N_0=P_1F_1'+F_1'N_0,$$
$$c=f_1'+e=(f_1'f_2'-f_1f_1'-f_1\Delta)/(f_2'-f_1).$$

But $\Delta=d-f_1'+f_2$. Therefore,

$$c=(f_1'f_2'-f_1f_2-f_1d)/(f_2'-f_1). \qquad (4b)$$

Now $P_1N_0+N_0P_2=P_1P_2$. Hence

$$c-c'=d$$

and

$$c'=c-d=(f_1'f_2'-f_1f_2-f_2'd)/(f_2'-f_1). \qquad (4'b)$$

By means of Eqs. (1), (2), (3) and (4a) or (4b) the positions of the cardinal points and the optical center of a thick lens can be calculated. The equations are also immediately applicable to a pair of thin lenses separated by a distance d or to a pair of thick lenses, the distance d denoting in this case the distance $H_1'H_2$ (measured from H_1').

The distances h, h', f, f', p, p' and c may be related directly to the optical and geometric constants of the lens or combination by substituting for f_1, f_1', f_2, f_2' and Δ their values in terms of radii of curvature, refractive indexes and the distance d. For purposes of arithmetical calculation, however, it is simpler to evaluate first the component focal lengths and optical interval and then to use Eqs. (1), (2), (3) and (4). On the other hand, important generalizations may be deduced from the extended expressions as in the following examples.

Expressing the focal lengths in Eq. (4b) in terms of radii of curvature and refractive indexes, we obtain for a thick lens,

$$c=\frac{r_1[n_1(n_3-n_2)d-n_2(n_1-n_3)r_2]}{n_1(n_3-n_2)r_1+n_3(n_2-n_1)r_2}.$$

If the lens is in one medium the equation reduces to $c=r_1d/(r_1-r_2)$; thus the position of the optical center is independent of the optical properties of the lens material and is the same for light of all wavelengths.

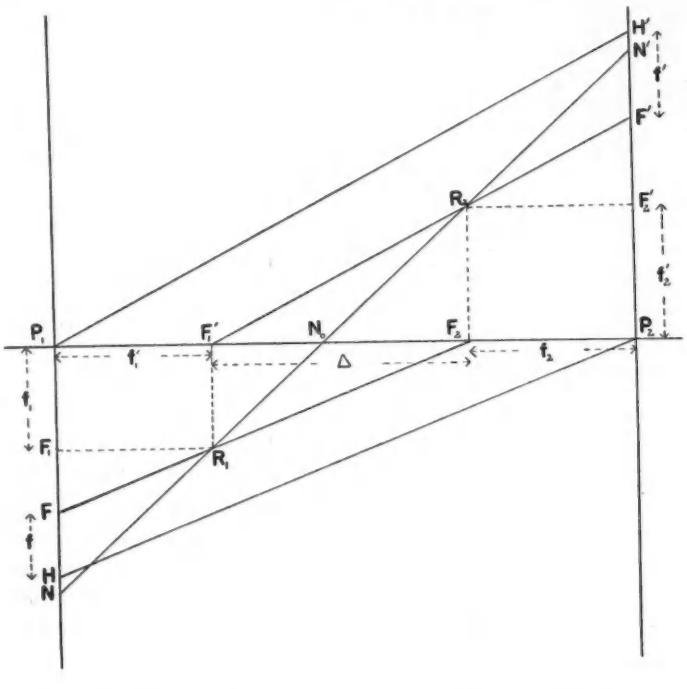

FIG. 9. Simplified construction diagram for a lens whose optical properties are similar to those of the lens to which Fig. 4 relates. In Fig. 9, $f'>f$; in Fig. 4, $f'<f$.

For a pair of thin lenses in one medium, of refractive indexes n_1 and n_2 and surface radii r_1, r_2 and r_3, r_4,

$$c=\frac{-r_1r_2d/(n_1-1)(r_2-r_1)}{\dfrac{r_1r_2}{(n_1-1)(r_2-r_1)}+\dfrac{r_3r_4}{(n_2-1)(r_4-r_3)}}.$$

If $n_1=n_2$,

$$c=\frac{-r_1r_2(r_4-r_3)d}{r_1r_2(r_4-r_3)+r_3r_4(r_2-r_1)},$$

and we again see that the position of the center is independent of the wavelength.

Forthcoming Iowa Colloquium of College Physicists

The annual Colloquium of College Physicists will occur at the University of Iowa on June 12, 13 and 14, 1947. The features of the meeting will be the exhibit of new devices for the lecture room and laboratory, invited papers on nuclear physics, high voltage accelerators, electronics and optics, cosmic rays, low temperature physics, short-wave electronic tubes, the social significance of science teaching, round table of experiences with physics courses in general education, and additional luncheon and dinner talks. Reservations should be made by addressing Professor G. W. Stewart, Hall of Physics, Iowa City, Iowa.

The Meaning of W/g

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THE statement of Newton's second law and the definitions of the absolute units of force —the dyne and the poundal—are pretty much the same in the many textbooks of general physics. But the descriptions of the gravitational system and the definitions of the engineering units of force and work are very different in different books. To determine the meaning of W/g in the equations $F=(W/g)a$ and $E=\frac{1}{2}(W/g)V^2$, it is necessary to use specific definitions of the weight W and the acceleration g. In order to use these terms properly and acceptably we will take their recognized engineering meanings from Timoshenko and Young.[1] These men are engineering authorities, and their book represents standard and reputable engineering practice. Other writers may begin with the definitions of different quantities; but many agree with Timoshenko and Young, and what we take from their *Engineering Mechanics* may fairly be called the British Engineering System.

In brief, these authors, as well, of course, as many others, base their system of dynamics on the equation

$$a/g=F/W.$$

This means that the acceleration a, imparted to a body B by a force F, is to the acceleration g, of any body in free fall, as the force F is to the weight W of the body B.

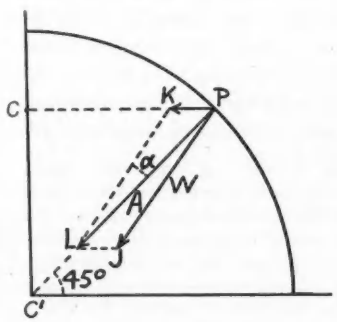

FIG. 1. Weight W and gravitational force A on a particle at latitude 45°N.

The weight W differs a little from the gravitational attraction exerted on the body B by the earth, since the earth's attraction produces an acceleration toward the center of the small circle giving the latitude of the body, as well as the acceleration of free fall. The weight W is, however, the force indicated by a correctly calibrated spring balance if it supports the body at rest or in equilibrium with respect to the earth. One may say that the vector sum of the two forces acting on such a suspended body—namely, the pull of the balance and the gravitational pull of the earth—give the magnitude and direction of the product ma_c, where a_c is the acceleration directed toward the center of the latitude circle. Here, of course, we are ignoring the acceleration of the earth in its orbit around the sun. Hence W is exactly the gravitational attraction only on a body at one of the poles of the earth, though W is in the direction of that attraction at the equator also.

In Fig. 1 P is a particle of mass m on the surface of the earth of latitude 45°N. The vector PL is A, the gravitational attraction of the earth; PJ is what we have called the weight W; and PK is ma_c, a force directed toward C, the center of the latitude circle. The important distinction between W and A is emphasized by a very good elementary problem. Assuming that the earth retained its present size and shape, what would its period have to be if a body, released at latitude 45°N on its surface, were to fall horizontally, straight south?

In the equation $a/g=F/W$, a and g may be expressed in any units whatever, but commonly they are in the same units, as we will take them to be. Then F is given in whatever units are used for W. This equation does not indicate what units are to be used. The weight W and therefore the force F may be in any units whatever. This basic equation may be written in the form

$$F=(W/g)a. \qquad (1)$$

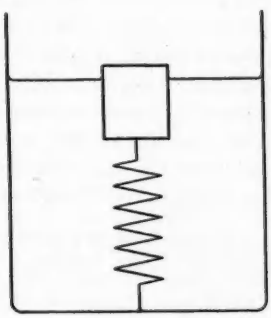

FIG. 2. An experiment involving free fall.

[1] Timoshenko and Young, *Engineering mechanics* (McGraw-Hill, ed. 2, 1940), pp. 258–260.

In statics and in hydrostatics it is a great convenience to use the pound as a unit of force. Nothing is gained, to be sure, by calculating the force against a dam in poundals. Moreover, the weight of a body in pounds force is numerically equal to its mass in pounds, and if an iron block is stamped "3" for pounds mass, the body that supports it (at rest with respect to the earth) exerts upon it a 3-lb force. This is a force numerically equal but opposite to PJ in Fig. 1. This figure of course is not drawn to scale. The mean solar day being 86,400 sec, the period of rotation of the earth is one sidereal day, or 86,164 sec. Then the angle $PLK=\alpha=6'0''$; and if the mass of particle P is 1 lb, then the three forces are, in poundals, $KP=0.07941$, $PJ=W=32.174$, $PL=A=32.2301$.

It is natural then for the engineer to express weight in pounds, this unit of force being nearly the same as the pull of the earth on a 1-lb object. The weight W being thus expressed, Eq. (1) gives F in pounds force. The authors quoted say also,[2] after setting forth the variation in the acceleration of free fall with location on the surface of the earth, "Thus W and g are varying with locality in the same proportion, and their ratio, which defines the mass of the body, remains constant." It is obvious from what has been said, in connection with Fig. 1, that the acceleration of free fall and the gravitational attraction do not vary "in the same proportion" from place to place. Accordingly we take W to be the local weight of a body, a number of local pounds force numerically equal to its mass, and g to be the local acceleration of free fall. It is only in local pounds that the force exerted by a body at rest is numerically equal to its mass in pounds. We are following *an* engineering system certainly, if not *the* engineering system, when with Timoshenko and Young we take W in local pounds weight. Further along we shall consider standard weights and an absolute gravitational system, but now we turn to the question: What is the meaning of W/g in Eq. (1)? Of course W/g will have the same meaning, whatever it turns out to be, in the derived equation $E=\frac{1}{2}(W/g)V^2$.

Let us set down for comparison the expression for force caused by electric charges. Consider two point electric charges q_1 and q_2 separated by a distance d in a vacuum. Then the Coulomb law gives for the force exerted by either particle upon the other, $F=q_1q_2/d^2$. We next define the field intensity, or field strength, E_1 at the location of q_2 and resulting from q_1 by the equation $F=E_1q_2$. These are two expressions for the same force. The field intensity is force per unit charge, and its cgs electrostatic unit is one dyne per cgs electrostatic unit of charge.

Now we may think of the weight W of a body on the surface of the earth as that component of the earth's gravitational pull which imparts to the body the acceleration of free fall g when the body is released, and which is the same, in magnitude and direction, as the force exerted by the body on its support when it is in equilibrium. Then, since W is equal to mg, we may call g the "weight per unit mass." The particular value of g characteristic of a certain locality is the weight of any body at that place divided by the mass of that body. Clearly the quantity g thus defined is not the intensity of the earth's gravitational field, but differs from that intensity just as the weight W differs from the total force A (Fig. 1) exerted by the earth. However, all bodies at the same place have the same weight per unit mass.

We say too often that "g is the acceleration due to gravity," and not often enough that "g is the weight per unit mass." Indeed, I think that the efforts directed, ever since physics was first taught, toward getting students to call g "acceleration due to a gravity" have been directed wrongly and wastefully. We should have been teaching them to say "g is the weight per unit mass." In many, if not most, of our problems in dynamics the weight per unit mass is more useful, more relevant, and certainly more meaningful than the acceleration of free fall. Why drag the acceleration of a freely falling body into a situation in which no body is freely falling? Why use this acceleration in a problem where nothing falls at all? Let me cite a few obvious examples.

If a block of iron of mass m is drawn with constant velocity along on a horizontal board where the coefficient of friction is μ, the horizontal force necessary is μ times the weight, and the weight is the mass times the weight per unit mass, so that $F=\mu mg$. In this situation there is no acceleration, and g should not be called an acceleration.

In the operation of the Atwood machine, let F be the tensile force in the cord on the side where the suspended mass is $M+m$. Since g is the weight per unit mass, the downward force on this load is $(M+m)g$ and, by Newton's

[2] Reference 1, p. 260.

second law,

$$(M+m)g-F=(M+m)a,$$

where the actual acceleration of the bodies is a. Similarly on the other side, where the suspended mass is M, if friction and the inertia of the cord and pulley are neglected,

$$F-Mg=Ma.$$

These two equations determine F and a. There is no acceleration g in this situation, and to call g an acceleration results in unnecessary difficulty and confusion.

Suppose that m is the mass of a particle suspended from a fixed point by a fine thread, so as to approximate the ideal pendulum. The restoring force, in the direction of motion (or opposite), is $mg\sin\theta$, or $mg\theta$ if θ is small, where g is the weight per unit mass of any body at the locality concerned. Here again it confuses the argument to call g an acceleration.

In these cases and in many others one arrives most directly and simply, and, I am sure, for the beginning student most easily and most surely, at the essential relations involved, by taking g to be the weight per unit mass. The case is even more clear when we come to calculate a centripetal force. The force in a horizontal cord when it constrains a body of mass 5 lb to make 10 rev/sec in a circle of radius 1 yd will be the same anywhere on the earth, or on the moon, or even in free space 10^{10} mi from either one. The use of g with any meaning whatever in this problem, as in the expression $(W/g)\omega^2R$, is a highly sophisticated procedure—unnecessary, obscure and hence repugnant to the student and to any teacher not dulled by repetition and familiarity. The use of g in the formula for the velocity of a transverse wave in a cord is, if possible, even more confusing, because obviously the acceleration caused by gravity has no effect at all upon the velocity to be found. Here $V=\sqrt{(F/m)}$, and if weight is brought into this expression the acceleration must be cancelled out by the introduction of g. This is an underhanded process never clearly understood.

A successful lecture experiment illustrates the case where free fall actually occurs, but where, nevertheless, g is advantageously taken as weight per unit mass. As indicated in Fig. 2, to the bottom of a clear glass jar of capacity 6 to 10 lit fasten one end of a coiled wire spring (or a rubber band), and fasten a large cork to the other end. Let the spring be a fairly weak one, of such length that when the jar is nearly full of water it is extended somewhat and the cork is perhaps nine-tenths submerged, that is, considerably lower than it would be if floating freely. While the demonstrator holds the jar in his hands two or three students are asked to stand very near by and closely observe the cork. Then the jar is dropped. It is easy to allow the jar to fall freely for two or three feet, but to check it before it crashes on the floor. The observers are to say what happened to the cork when the jar was released, and during the free fall. Probably they will not agree; but the fact is that as soon as the fall begins the cork goes beneath the surface, and it stays submerged as long as free fall continues. When the fall is checked the cork emerges at once.

The explanation obviously is as follows. While the jar is motionless the cork is in equilibrium under the action of three forces—weight of the cork, pull of the extended spring, and buoyant force due to the weight of the displaced water. However, when the jar is dropped the buoyant force immediately disappears. The downward force on any particle of water of mass m is mg, where g is the weight per unit mass. Motion, whether accelerated or not, makes no change in this force. But while the jar and water are falling freely, the weight mg of the particle imparts an acceleration g to it, and there is no force left over to produce hydrostatic pressure. Hence, during free fall there is nothing to sustain the cork, and it is pulled beneath the surface by the spring. If free fall could continue the cork would go down so far that the spring was no longer extended.

We must now inquire about the units in which g, the weight per unit mass, is properly expressed. A conventional and familiar unit of mass is the pound; it is legally defined in terms of the international kilogram. The English unit of acceleration is 1 ft sec^{-2}. Then, since the weight of a body gives that body an acceleration of about 32.2 ft sec^{-2}, and 1 poundal, by definition, gives a body of mass 1 lb an acceleration of 1 ft sec^{-2}, and g is 32.2, nearly, g must be about 32 poundals per pound.

The preference of some writers for the engineering units appears to be caused by their aversion to the poundal. Yet the poundal is just as simple and respectable as the dyne, to which even the engineers are reconciled. Everyone who

uses the equation $F=(W/g)a$ is using the poundal implicitly. If g is numerically 32.2, it is unavoidably the weight per unit mass in poundals per pound. This being the case, what finally is the meaning of W/g?

We consider a certain body B at the location L. Then

$$W/g=\frac{\text{Weight of } B \text{ at } L \text{ in pounds force}}{\left(\frac{\text{Weight of } B \text{ at } L \text{ in poundals}}{\text{Mass of } B \text{ in pounds}}\right)}.$$

Therefore,

$$W/g=\frac{\text{Mass of } B \text{ in pounds}}{\left(\frac{\text{Local weight of } B \text{ in poundals}}{\text{Local weight of } B \text{ in pounds force at } L}\right)}.$$

But the fraction in the denominator is an abstract number, and equal to about 32.2. Therefore, the equation $F=(W/g)a$ means just the same thing as the equation $F=Kma$, K being a numerical factor introduced to convert the product ma, which gives poundals, to the number of pounds force. Similarly, $E=\frac{1}{2}(W/g)V^2$ is merely $E=\frac{1}{2}KmV^2$, K being the numerical factor by which the number of foot poundals (given by $\frac{1}{2}mV^2$) is converted to the number of foot pounds.

It is easy, of course, to convert the gravitational system into what is, in reality, an absolute system. We can define a standard pound force as the weight of a body of mass 1 lb at some particular place—commonly at sea level and latitude 45°N. Here the acceleration of a body in free fall is g_n, and hence 1 standard pound is equivalent to g_n poundals. Force having certain dimensions, whether expressed in one unit or another, we are using g_n as an abstract number, namely 980.665, the number of dynes equivalent to one standard gram of force, or 32.174, the number of poundals equivalent to one standard pound of force.

This brings us to exact agreement with Millikan, Roller and Watson[3], who write

$f(\text{gwt})=1/g_n m(\text{gm})\cdot a(\text{cm sec}^{-2})$, $g_n=980.665$;
$f(\text{lbwt})=1/g_n m(\text{lb})\cdot a(\text{ft sec}^{-2})$, $g_n=32.174$;

and continue, "it is to be noted that g_n is used in the last two equations to denote pure numbers. . . ." In the second equation, for example, if both m and a are unity, then $f=1/32$ of 1 pound force, obviously correct, and similarly correct in the first case.

The instructions given by Lindsay[4] lead to this procedure exactly, for he says "the reader is advised always to get the force in poundals first, and then if necessary to change to pounds by dividing by 32.2." No doubt he would advocate the corresponding method for calculating kinetic energy.

It seems that the pound mass is used in the engineering system to define the pound force, and then the pound mass is repudiated, and advocates of the system build it up on force as a fundamental quantity, ignoring the fact that mass and the pound mass were necessarily used in the original foundation.

There is great variety among the accounts of the engineering units in the different textbooks of general physics, and particularly in the treatment of rotation. One widely used book employs the equation $F=(W/g)a$ for the force required to produce translation, using W to represent weight in pounds force. But the same book expresses moment of inertia, $I=\Sigma mr^2$, in pounds feet squared, and kinetic energy of rotation as $\frac{1}{2}I\omega^2$, without declaring what units will be given for energy when I is in pounds feet squared and ω is in radians per second. Apparently here the pound mass and the foot poundal of energy, though not tolerated in translation, are legitimatized by rotation. Some other books are more thoroughgoing and give moment of inertia as $(W/g)k^2$, where k is the radius of gyration, so that the unit of moment of inertia is 1 lb ft sec^2, that is, $[FLT^2]$. Here the pound is the pound force. But, it seems too bad to drag force into moment of inertia, an entirely passive and geometric idea, only to have to bring in time squared also, in order to cancel the time squared that is inherent in any unit of force. For moment of inertia does not depend upon force at all, nor upon time either. This procedure must remind Alice, Jr. of the white knight and his fan.[5] The knight may have been a little more absurd, but he has made much less trouble.

[3] *Mechanics, molecular physics, heat and sound* (Ginn, 1937), p. 37.

[4] *General physics* (Wiley, 1940), p. 62.

[5] L. A. Hawkins and S. A. Moss, "Alice and the sluggers," *Am. J. Physics* **13**, 405 (1945).

Physics of Rockets: Dynamics of Long Range Rockets*

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Sounding Rockets and Escape from the Earth

39. History of High-Altitude Rockets

THE term "long-range rocket" is used to denote a vehicle whose range is at least 100 mi and which is propelled by a rocket whose burning time is, say, 30 sec or more, the primary objective of the flight being the attainment of the longest possible range. These characteristics serve to distinguish this type of vehicle from the so-called artillery rocket, designed to carry a large payload over a short distance. A "sounding rocket" is a special type of long-range rocket, the purpose of which is to carry scientific instruments to extreme altitudes in a vertical trajectory, to determine the physical conditions prevailing in the upper reaches of the atmosphere and, indeed, beyond the earth's atmosphere. Since meteorological balloons have penetrated the atmosphere to a maximum altitude of only 25 mi, the useful region for the sounding rocket begins at that height.

The first technical discussion on record in which this problem is considered is that of Ziolkowsky, published in Russian in 1903.[27] From his calculations he concluded that the potentially superior performance of liquid propellants as compared with powder-type propellants warranted research in that direction.

The earliest engineering attempts to achieve high altitudes by means of rockets were made by R. H. Goddard[28] in the United States, starting about 1912 and continuing until about 1941. After determining the exhaust velocities obtainable from solid propellants, such as black powder and smokeless powder, and from the oxygen-gasoline combination, he designed and launched several liquid-propellant rockets. The maximum altitude—7500 ft—was reached in 1935 in the flight of a 14-ft rocket propelled by an oxygen-gasoline unit (Fig. 42). One of the interesting contributions of this work was the means of stabilizing the vehicle with gyroscopically controlled vanes acting intermittently in the jet blast.

In Germany, Hermann Oberth[29] developed much of the theory of rocket performance during the period 1923 to 1930. Active experimental work was conducted by the *Verein fur Raumschiffahrt* (Society for Space Travel), founded in 1927 to promote the development of the field of rocketry. Under its auspices, a number of liquid oxygen-gasoline rockets were developed and flight-tested. The maximum altitude achieved in any of these flights was about 3000 ft. This volunteer society disbanded in 1932, when the program was taken over by the German Ordnance Department.[30]

A similar volunteer group, organized in the United States in 1931, is now called the American Rocket Society. Although its original objective was the development of high-altitude sounding rockets, it had progressed only to the point of developing an oxygen-gasoline rocket motor

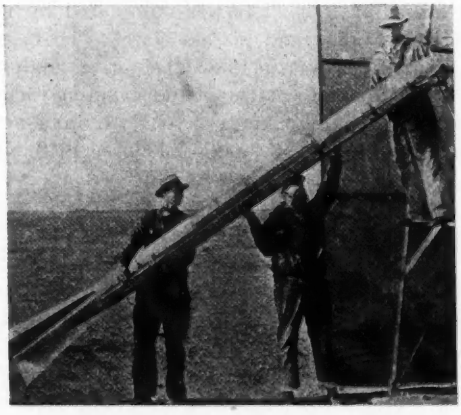

FIG. 42. The original liquid oxygen-gasoline rocket of R. H. Goddard, which achieved an altitude of 7500 ft in 1935.

* The preceding articles [*Am. J. Physics* **15**, 1, 121 (1947)] dealt with principles of rocket propulsion and with solid-propellant and liquid-propellant rockets.

[27] K. E. Ziolkowsky, "Rockets in cosmic space," *Sci. Survey* (1903).

[28] See reference 1 in our first paper; also R. H. Goddard, "Liquid-propellant rocket development," *Smithsonian Misc. Collections* **95**, No. 3 (1936).

[29] H. Oberth, *Wege zur Raumschiffahrt* (Munich, 1929).

[30] W. Ley, *Rockets* (Viking, 1941).

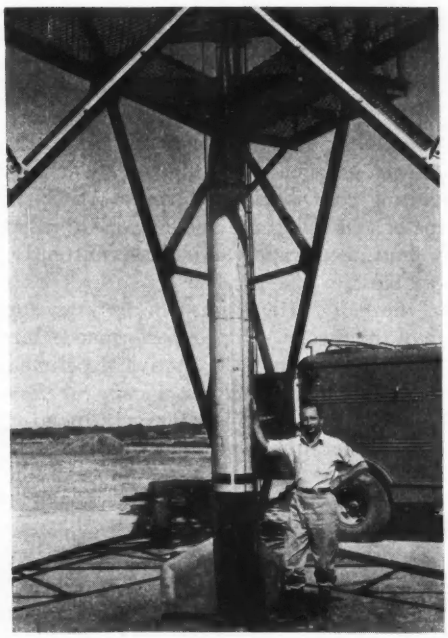

FIG. 43. The WAC Corporal sounding rocket being loaded into the launching tower.

FIG. 44. The flight of a solid-propellant rocket used in the preliminary development of the WAC Corporal sounding rocket. The smoke trail shown is about 3 mi long.

before our entrance into World War II interrupted its activities.[31]

At the California Institute of Technology, interest in the possibility of achieving altitudes in excess of 100,000 ft was stimulated by the work of Malina and his associates, who began their investigations in 1936. Theoretical analyses of the flight performance of the sounding rocket and the thermodynamics of rocket motors were carried out during the period 1936 to 1940, and the characteristics of various propellants were investigated.[32]

In 1940 the program at the California Institute of Technology was expanded and directed toward rockets for war purposes. Interest in sounding rockets was actively revived in 1945, with the development of the so-called WAC Corporal sounding rocket, a vehicle designed specifically to carry about 25 lb of instruments to high altitudes. Ten units were fired in August of that year, attaining a maximum altitude of 230,000 ft, or 44 mi[33] (see Figs. 40 and 43). The firing of a solid-propellant test vehicle used in preliminary development of the WAC Corporal is shown in Fig. 44.

The highest altitude to date was reached by the German V-2 rocket missile when, in 1944, two units were fired vertically upward to a height of 107 mi.[34] In the normal ground-to-ground trajectory the peak of the flight is about 60 mi high. In recent experimental lauchings of captured V-2 rockets at the Army Ordnance Proving Ground, White Sands, New Mexico, altitudes in excess of 100 mi have been achieved. This device represents the most advanced technic of rocket-vehicle development at the present time (see Figs. 45 and 46).

[31] G. E. Pendray, *The coming age of rocket power* (Harpers, 1945).

[32] See reference 2, first paper; also F. J. Malina and A. M. O. Smith, "Flight analyses of the sounding rocket," *J. Aero. Sci.* **5**, 199 (1938); H. S. Tsien and F. J. Malina, "Analysis of a sounding rocket propelled by successive impulses," *J. Aero. Sci.* **6**, 50–58 (1938).

[33] F. J. Malina, "Is the sky the limit?," *Army Ordnance* **31**, No. 157, 45 (1946).

[34] A. V. DaRosa, "Analysis of V-2 performance," *Aero. Digest*, p. 98 (May 1, 1945).

40. General Equations of Motion of a Rocket

In this section the dynamics of the upward flight of a sounding rocket will be analyzed. By examining the influence of the various parameters that determine flight performance, the future possibilities of the sounding rocket can be estimated.

A rocket-propelled vehicle such as the V-2 or the WAC Corporal consists essentially of the following components: the propulsion system, a so-called payload that in a sounding rocket comprises the physical instruments being carried aloft, a quantity of liquid propellants, and the necessary shell and structure. The liquid propellants generally represent the largest portion of the gross mass of the vehicle. In the manner of the V-2, the rocket is set up on end, pointing vertically upward, for firing. If the rocket is not equipped with automatic guidance apparatus, it must be projected from a guiding tower with sufficient velocity to be stabilized by aerodynamic forces.

In the following analysis only the flight performance of the rocket will be treated. Other aspects of the dynamical problem, such as flight stability, the effects of control forces, and the stability of control mechanisms, are too specialized for presentation in this paper.

During powered flight, the rocket is accelerated by the thrust delivered by the rocket motor, which must be sufficient to overcome the retarding forces due to gravity and aerodynamic drag (Fig. 47). If the altitude at the end of powered flight is small compared with the radius of the earth, the variation of gravity may be neglected. However, the mass of the rocket, the aerodynamic drag and the thrust delivered by the motor may vary during the flight of the rocket.

If F denotes the thrust of the rocket motor, D the aerodynamic drag, and M the instantaneous mass of the rocket, the acceleration of the rocket is given by the relation

$$a = \mathrm{d}v/\mathrm{d}t = (F - D - Mg)/M. \tag{94}$$

It is usual for the propellant flow controls of a rocket to be so designed that the mass of liquid ejected per unit time is constant. In this case the mass of the rocket decreases linearly with time, in accordance with the equation

$$M = M_0(1 - \zeta t/t_p), \text{ for } t \leqslant t_p \tag{95}$$

where M_0 is the initial mass of the rocket, t_p the duration of powered flight, and ζ the ratio of the initial mass of propellant in the rocket to the gross mass of the rocket.

The thrust at any time is expressed by the relation:

$$F = M_p c/t_p, \tag{96}$$

where $M_p[=\zeta M_0]$ is the initial mass of propellant, and c is the effective exhaust velocity of the jet. The magnitude of c is a function of altitude, as explained in SEC. 10, but to simplify the mathematics a constant average value of c may be used in Eq. (96) and in the remainder of the analysis. Therefore, although the thrust may be as much as 25 percent larger at the end of burning than at the start, an average constant thrust will be assumed here.

The drag D is determined from the equation

$$D = C_d A \cdot \tfrac{1}{2}\rho v^2, \tag{97}$$

FIG. 45. A German V-2 rocket just after take-off at White Sands Proving Grounds, New Mexico.

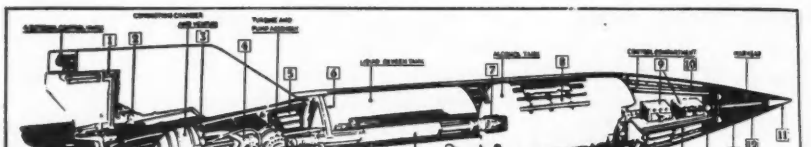

FIG. 46. Official British press illustration of the German V-2 rocket, dated Dec. 8, 1945.

where C_d is the drag coefficient based on the frontal cross-sectional area A, and ρ is the atmospheric density. The drag coefficient depends not only on the size, proportions and surface qualities of a particular vehicle, but also on the velocity v. Its dependence on v is expressed in terms of its dependence on the Mach number, the ratio between flight velocity and the velocity of sound under local conditions. To simplify the considerations in this paper an average constant value of C_d will be assumed.

Combination of Eqs. (94) to (97), and integration of the resulting equation of motion yields the expression for the velocity at any time t during powered flight:

$$v=\int_0^t \frac{\zeta c/t_p}{1-\zeta t/t_p}\mathrm{d}t-\int_0^t g\mathrm{d}t - \int_0^t \frac{(C_d/\mu)\cdot\frac{1}{2}\rho v^2}{1-\zeta t/t_p}\mathrm{d}t+v_0, \quad (98)$$

where v_0 is the initial or launching velocity. The parameter μ has been introduced to denote the ratio M_0/A, the magnitude of which determines the relative importance of the drag term. The first two integrals can be integrated directly. The third integral may be calculated by numerical methods, and its value is denoted by q_1C_d/μ, where

$$q_1=\int_0^{t_p}\frac{\frac{1}{2}\rho v^2}{1-\zeta t/t_p}\mathrm{d}t. \quad (99)$$

The velocity at the end of powered flight is then found to be

$$v_p=-c\ln(1-\zeta)-gt_p-q_1\frac{C_d}{\mu}+v_0. \quad (100)$$

In the case of drag-free flight, the velocity at the end of powered flight becomes

$$v_{p0}=-c\ln(1-\zeta)-gt_p+v_0. \quad (101)$$

By integrating Eq. (98) again, the altitude at the end of powered flight, h_p, can be obtained; it is

$$h_p=ct_p\left[1+\frac{(1-\zeta)}{\zeta}\ln(1-\zeta)\right] -\tfrac{1}{2}gt_p^2+v_0t_p+h_0-q_2\frac{C_d}{\mu}, \quad (102)$$

where q_2 is the double integral with respect to t of the integrand of q_1. The effect of drag can be neglected by omitting the last term, q_2C_d/μ.

At the end of powered flight the vehicle continues to coast until its velocity is reduced to zero. The gain in altitude during coasting flight, h_c, if it is assumed to take place above the atmosphere, is simply

$$h_c=\tfrac{1}{2}v_p^2/g. \quad (103)$$

The altitude h at the top of the trajectory is the sum of h_p and h_c; that is,

$$h=\frac{c^2}{2g}[\ln(1-\zeta)]^2+ct_p\left[1+\frac{1}{\zeta}\ln(1-\zeta)\right] +\frac{v_0^2}{2g}+h_0-\frac{v_0c}{g}\ln(1-\zeta) -\frac{C_d}{\mu}\left[q_2+\frac{v_0q_1}{g}-\frac{cq_1}{g}\ln(1-\zeta) -q_1t_p-\frac{q_1^2}{2g}\frac{C_d}{\mu}\right]. \quad (104)$$

The significance of each of the terms in Eq. (104) may now be examined. The first term, which is usually the predominant one, is proportional to the square of the exhaust velocity. Because of the logarithmic factor, it is very sensitive to small changes in the propellant mass ratio ζ, especially for values of ζ approaching unity. In fact, no matter how small the exhaust velocity, the altitude becomes infinite as the nonexpendable mass of the rocket approaches zero. The second term is always negative, and since it is proportional to t_p, the longer the burning time, the lower the altitude of the peak of the flight. If t_p were the only variable, the maximum altitude would be attained if all the propellant were fired instantaneously. However, a rocket designed to withstand the resulting large forces would require a much larger ratio of structural mass to propellant mass, with the result that the first term in Eq. (104) would be greatly reduced. Also, for flights starting from the earth's surface, a high velocity at the start of flight would increase the effect of drag. For these reasons, there is generally an optimum burning time that must be determined for each type of rocket. The third, fourth and fifth terms are correction terms to be applied only when the rocket is started with an appreciable velocity before firing, as when booster rockets are used, or when the flight takes place from an elevated launching site.

The last term, negative in sign, involves the retardation caused by drag. It vanishes, of course, for $C_d=0$, or $\rho=0$, that is, in vacuum or above the atmosphere. It is significant that the term becomes negligible for large values of μ. The parameter $\mu[=M_0/A]$ is proportional to the product of the length of a rocket and its average density. Thus, if similar rockets are compared, the effect of drag becomes negligible as the size of the rocket increases. Therefore, the performance of large rockets can be closely approximated by the first five terms alone, which contain no factors depending on size.

The altitude reached by a rocket large enough so that the last term vanishes when launched from a stationary position at $h_0=0$ is given simply by the equation

$$h=\frac{c^2}{2g}[\ln(1-\zeta)]^2+ct_p\left[1+\frac{1}{\zeta}\ln(1-\zeta)\right]. \quad (105)$$

The relative effects of the exhaust velocity c, the propellant mass ratio ζ, the firing time t_p and the gross mass per unit frontal area μ are illustrated in the graphs of Figs. 48 and 49, representing altitudes calculated by means of Eq. (104). These calculations were carried out with the aid of the drag curve in Fig. 50—a curve considered representative for well-designed rocket vehicles. The atmospheric density and temperature distributions of the National Advisory Committee for Aeronautics (NACA) standard atmosphere were used.[35]

The characteristics of both the WAC Corporal and the V-2 rocket are presented in Table IX for comparison of actual performance with that predicted by means of Eq. (104), using the drag characteristics shown in Fig. 50. The disagreements in both cases are probably caused by such effects as the increase in drag during flight because of slight yawing motions, inefficient utilization of propellant in the starting and stopping periods, and so forth. In particular, the drag coefficient of the WAC Corporal is known to be larger than that given in Fig. 50. The exhaust velocity given for the V-2 rocket has been corrected for the drag acting on the steering vanes located in the jet.

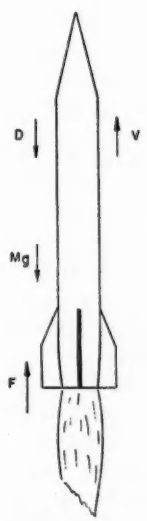

FIG. 47. The forces acting on a rocket in vertical flight through the atmosphere.

[35] W. G. Brombacher, "Altitude-pressure tables based on the United States standard atmosphere," NACA Report No. 538 (1935).

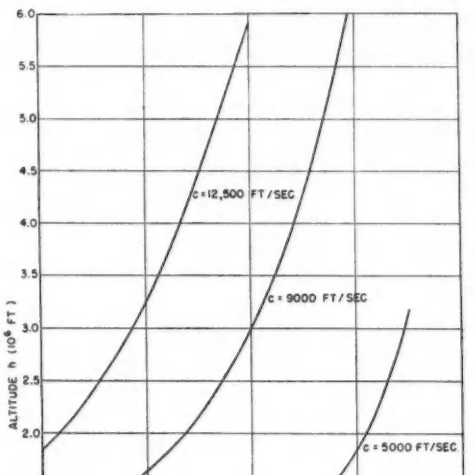

FIG. 48. The effect of propellant mass ratio and effective exhaust velocity on the altitude attained by a sounding rocket in vertical flight. The influence of air drag has been neglected, and it has been assumed that $\mu=\infty$, $t_p=30$ sec, $h_0=0$ and $v_0=0$.

41. Considerations of Flight into Space

The subject of space flight involves many diverse problems, including trajectory control, navigation, communication, and take-off and landing technics. The basic physical question. however, concerns the means for imparting the necessary energy to the vehicle so that it may escape the gravitational grasp of the earth. Although some consideration has been given to the gun-like expulsion of a free piston from a tube, investigators are agreed that the only feasible method is the use of the rocket.[36a, 36b]

The energy or velocity requirements for escape from the earth will here be examined, and then the design of rockets capable of producing such velocities will be considered. Four cases are of interest.

[36a] H. Lorenz, "The possibility of space travel," *Ver. Deut. Ing.* **71**, 651–654 (1927); [36b] F. J. Malina and M. Summerfield, "The problem of escape from the earth by rocket," *Proc. Sixth Intern. Congr. Applied Mechanics* (1946). (To be published in Journal of Aeronautical Sciences.)

(i) An earth satellite, permanently revolving in a circular orbit just outside the atmosphere of the earth, could remain in such an orbit indefinitely without requiring the expenditure of additional energy, unless struck by a meteorite.[37] The distance of the orbit from the earth should be sufficient that the satellite may escape the drag of the atmosphere. The velocity of the satellite must be such as to balance the gravitational attraction of the earth, according to the following equations:

$$v^2/(R+h)=g_0R^2/(R+h)^2, \tag{106}$$

$$v=[g_0R^2/(R+h)]^{\frac{1}{2}}, \tag{107}$$

where R is the radius of the earth, h the altitude of the orbit above the earth's surface, and g_0 the acceleration due to gravity at the earth's surface. The period of one revolution is

$$T=2\pi(R+h)/v=2\pi(1+h/R)[(R+h)/g_0]^{\frac{1}{2}}. \tag{108}$$

The total energy per unit mass, E, that must be imparted to the vehicle to place it in the orbit is the sum of the kinetic and potential energies involved; that is,

$$E=\tfrac{1}{2}v^2+\int_0^h g_0\frac{R^2}{(R+x)^2}dx=\frac{v^2}{2}+g_0h\frac{R}{(R+h)}. \tag{109}$$

Substituting the value of v from Eq. (107) yields

$$E=\tfrac{1}{2}g_0R\left(1+\frac{h}{R+h}\right). \tag{110}$$

The possible contribution of the peripheral velocity of the earth's surface to the required total velocity has been omitted from these equations. The variation of the required total

TABLE IX. Comparative flight data for WAC Corporal and V-2 rockets.

Specifications	WAC corporal	V-2 rocket
Propellant type	Acid-aniline	Oxygen-ethanol
Propellant mass ratio	0.54	0.70
Average exhaust velocity (ft/sec)	6700	6880
Initial velocity (ft/sec)	720	0
Duration of firing (sec)	45	70
Gross mass (lb)	696	27,900
Frontal area (ft²)	0.8	23
Mass per unit area (lb/ft²)	850	1220
Observed altitude (mi)	44	107
Computed altitude (mi)	57	119

[37] F. L. Whipple, "Meteors and the earth's upper atmosphere," *Rev. Mod. Physics* **15**, 246–264 (1943).

energy with altitude is given in the last term of Eq. (110), from which it can be seen that the energy of an orbit at 200 mi altitude is only 5 percent more than that of one just above the earth's surface. The total energy per unit mass that must be imparted to the vehicle to place it in the 200-mi orbit is 3.56×10^8 ft lb/slug. The orbital velocity is 25,600 ft/sec, and the period of revolution is 1.51 hr.

Consideration of a project of this type necessarily raises many interesting questions that are not within the scope of this paper. These include the determination of the optimum trajectory for entering the orbit, the method of guidance or control of the flight path of the vehicle, the means for communicating with the vehicle while in flight, the hazard of destruction by meteoric bodies, and the method of subsequent descent. Much work and analysis are required before practical flights can be realized.

(ii) An interesting special case of the satellite rocket is the so-called "stationary satellite," the angular velocity of which is identical with the spin velocity of the earth. Such a vehicle, traveling in an orbit in the same plane as the equator, would appear stationary with respect to an observer on the earth. The possibility has been mentioned that a stationary satellite would be a convenient relay point for continuous short-wave communications with about half the surface of the earth. From Eqs. (107), (108) and (110) it can be shown that the stationary satellite will be located at a distance of 22,400 mi from the surface of the earth, about six earth-radii away, or one-tenth the distance to the moon. Its orbital velocity will be 10,200 ft/sec, and the total energy per unit mass required to place it in its orbit is 6.29×10^8 ft lb/slug.

(iii) Another example of interest is the calculation by the same method of the minimum energy that must be imparted to a vehicle to enable it to escape completely from the earth's gravitational influence. By means of Eq. (110) the total energy required is found to be 6.79×10^8 ft lb/slug, only slightly more than that of the stationary satellite. This energy corresponds to the familiar "escape velocity" of 7 mi/sec.

(iv) As a final case, escape from the earth and the entire solar system may be considered. The attractive force of the sun is sufficient to require

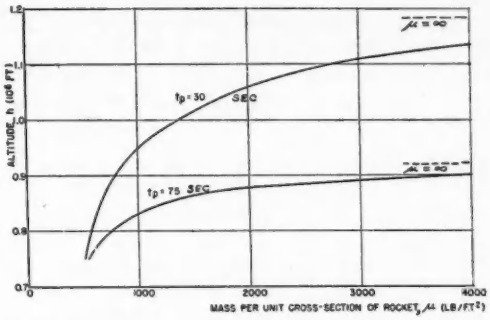

FIG. 49. The effect of thrust duration t_p and gross mass per unit cross-sectional area μ on altitude of vertical rocket flight. It has been assumed that $\zeta=0.70$, $c=7700$ ft/sec, $h_0=0$, $v_0=0$ and that C_d is given by Fig. 50.

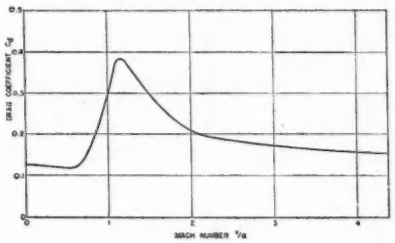

FIG. 50. Curve of drag coefficient *versus* Mach number used in calculating rocket trajectories. It is assumed here that no jet is acting, that the angle of attack is zero and that standard NACA atmospheric tables are applicable.

a considerable expenditure of energy to achieve complete escape. The influence of the planets is negligible since their combined mass is about one-thousandth that of the sun.

The energy per unit mass required to bring a vehicle from the surface of the earth to a position far outside the solar system is the sum of the increase in potential energy due to the earth's field plus the increase in potential energy due to the sun's field minus the kinetic energy corresponding to the orbital velocity of the earth. The first and second terms are GM_e/R and GM_s/S, where G is the gravitational constant, M_e is the mass of the earth, R is the radius of the earth, M_s is the mass of the sun, and S is the distance between earth and sun. The third term is $\frac{1}{2}GM_s/S$. The kinetic energy resulting from the earth's spin is neglected. The required energy per unit mass is then given by

$$E=\frac{GM_e}{R}+\frac{GM_s}{S}-\frac{\frac{1}{2}GM_s}{S}=\frac{GM_e}{R}\left[1+\frac{\frac{1}{2}M_s}{M_e}\frac{R}{S}\right], \quad (111)$$

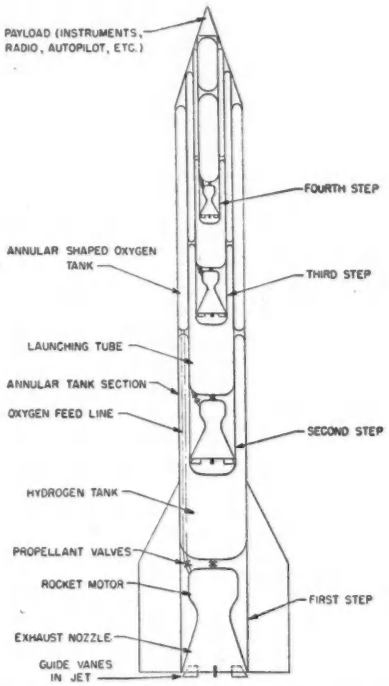

FIG. 51. One possible arrangement of components in a four-step rocket.

or, since $g_0 = GM_e/R^2$,

$$E = g_0 R\left[1 + \frac{\frac{1}{2}M_s}{M_e}\frac{R}{S}\right]. \qquad (112)$$

Now $M_s/M_e = 3.32\times10^5$, $R/S = 4.26\times10^{-5}$ and $g_0R = 6.79\times10^8$ ft lb/slug. Then the value of E becomes 54.8×10^8 ft lb/slug.

In the next two sections the characteristics of rockets capable of attaining the energies required in these four cases will be considered.

42. General Theory of Multiple-Step Rockets

The consideration of multiple-step rockets arises necessarily from the seeming difficulty of ever achieving light enough structure in individual rockets to attain velocities sufficient for escape from the earth. This may not apply, of course, to nuclear-energy rockets. In SEC. **41** it was shown that velocities of 25,000 ft/sec or more are required. Taking this figure, the necessary characteristics of escape rockets may be calculated by means of Eq. (101).

One of the highest exhaust velocities obtainable by means of a chemical reaction is that of the oxygen-hydrogen combination, about 10,500 ft/sec at sea level (see Table IV, first article). It can be assumed, in accordance with the discussion of SEC. **10**, that the average value of the exhaust velocity, during upward flight out of the atmosphere, may be as much as 20 percent greater than the sea-level value. This effect is a result of the reduced back pressure prevailing at upper altitudes, and its exact magnitude depends on the operating combustion pressure, the design of the nozzle, and the flight trajectory.

Inserting 12,500 ft/sec for c in Eq. (101), and assuming a burning time t_p of 100 sec, one finds that a rocket velocity of 25,000 ft/sec can be obtained only if the propellant mass ratio ζ exceeds 0.895. That is, the mass of the rocket and payload, excluding the propellant, may not exceed 10 percent of the gross mass. The higher rocket velocities required for cases (ii), (iii) and (iv) of SEC. **41** would necessitate even lighter structure. It is outside the scope of this paper to examine in detail the factors that enter into the structural mass of a rocket, but it should suffice to point to the V-2 rocket, for which ζ is 0.70, to appreciate the difficulty of developing a practical rocket vehicle having a value of ζ of 0.90 or more.

The velocity attainable with a single-step rocket is severely limited by the fact that propulsion energy must be utilized to accelerate continually the entire empty mass of the rocket even after the major portion of that empty mass is no longer useful. The idea therefore suggests itself that a rocket consisting of several steps be used, each of which operates independently and carries its own propellant load.[36b] As each step is exhausted it is dropped from the rocket, and the propulsion of the remainder is taken up by the next step. The payload is carried in the last, or smallest, step of the rocket (Fig. 51).

The performance of multiple-step rockets may be analyzed by the methods derived in SEC. **40**. Let the number of steps be N, each step being numbered in sequence so that the last step, the Nth, carries the payload of mass M_l. A parameter λ, called the payload ratio,[38] can be defined for

[38] The symbol λ was used in SEC. 9 in an entirely different sense.

each step as the ratio of the mass of the carried load or payload to the mass of the rocket at the moment when that step begins to fire. It has been shown[36b] that the optimum arrangement is that in which the values of λ for all the steps are equal. For example, in a three-step rocket, a 100-lb payload consisting of a radio and instruments would be carried within the body of the third step, and the gross mass of this step, including the payload, might be 500 lb. The payload ratio for the third step, λ_3, is 0.20. The third step is launched in flight from the second step, whose gross mass, including the third step as a payload, is 2500 lb. The value of λ_2 is also 0.20. The gross mass of the first step, which is really the entire rocket assembly, is 12,500 lb, so that the mass of the second step constitutes 0.20 of the total. In general, then, this assumption leads to the relation:

$$M_0^{(1)} = \lambda^{-N} M_l, \tag{113}$$

where $M_0^{(1)}$ is the initial mass of the first step—in other words, the gross mass of the rocket.

By definition, the gross mass $M_0^{(1)}$ of the first step includes the mass $M_0^{(2)}$ of the second step, the mass $M_p^{(1)}$ of propellants in the first step, and the empty mass $M_e^{(1)}$ of the first step. In general, for the nth step, this relation is

$$M_0^{(n)} = M_0^{(n+1)} + M_p^{(n)} + M_e^{(n)},$$

$$\text{for} \qquad 1 \leqslant n \leqslant N \text{ and } M_0^{(N+1)} = M_l. \tag{114}$$

It may be further assumed that all the steps in a given rocket can be designed with equal structural effectiveness. (It is believed that this is not far from the truth, except perhaps for small rockets of less than 100 lb mass.) This assumption can be expressed by defining a parameter ϵ, called the structural factor,[39] whose value is the same for each step:

$$\epsilon = \frac{M_e^{(n)}}{M_e^{(n)} + M_p^{(n)}} \quad \text{for } 1 \leqslant n \leqslant N. \tag{115}$$

The structural factor of the V-2 factor is 0.24, the lowest value that has been achieved in any rocket vehicle to date.

The propellant mass ratio ζ, defined in SEC. 40, is related to ϵ and λ by the equation

$$\zeta = (1-\lambda)(1-\epsilon). \tag{116}$$

[39] The symbol ϵ was used in SEC. 9 in a different sense.

Figure 52 illustrates schematically the various masses involved in the definition of λ, ϵ and ζ.

According to Eq. (101) the contribution of each step to the speed of the rocket is given by the equation

$$v_n - v_{n-1} = -c_n \ln[\epsilon(1-\lambda)+\lambda] - g_0 t_n, \tag{117}$$

where v_n is the velocity of the rocket at the end of burning of the nth step, c_n is the average value of the exhaust velocity during the firing of the nth step, and t_n is the firing period of the nth step. Because the initial steps of the rocket are generally large enough to reduce the drag-thrust ratio to negligible values, and the later steps operate above the atmosphere, the drag term is omitted from Eq. (117). It is assumed that the powered flight is essentially vertical, and that the sea-level value of g may be used for all steps without much error. Each step starts to fire immediately after the preceding step has ceased firing, and is discarded immediately after it is exhausted. A detailed treatment of the validity

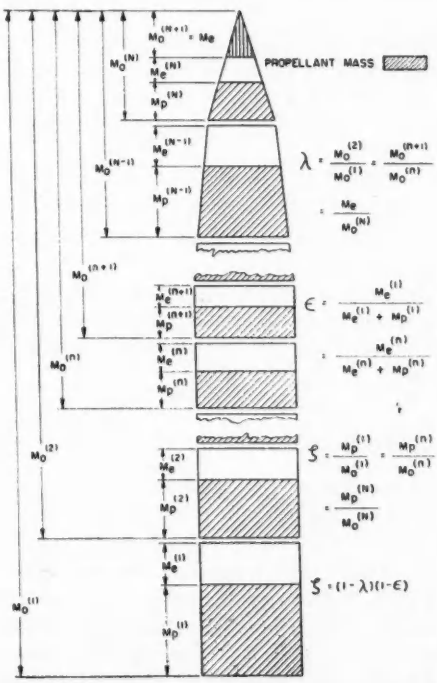

FIG. 52. Disposition of masses in a step rocket which enter into the definitions of the propellant mass ratio ζ, the structural efficiency factor ϵ and the payload ratio λ.

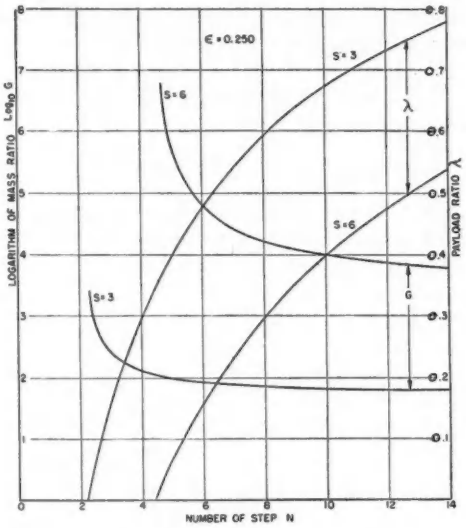

FIG. 53. Graph of the step-rocket payload ratio λ and over-all mass ratio G, showing the variation in λ and G as the number N of steps increases. Two typical values, 3 and 6, of the velocity ratio S, and a structural efficiency factor ϵ of 0.25, have been chosen.

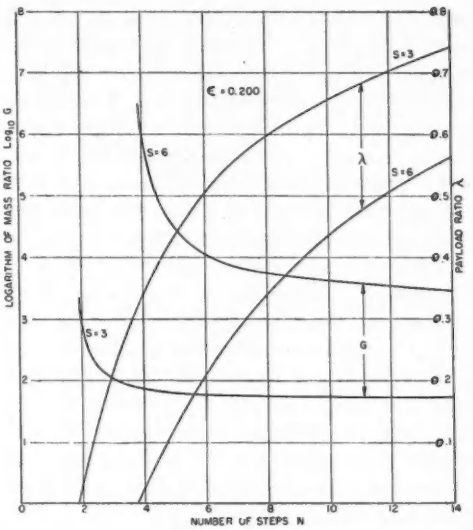

FIG. 54. Graph of payload ratio λ and over-all mass ratio G similar to Fig. 53, but with an improved structural efficiency factor ϵ of 0.20.

of these assumptions has been given by Malina and Summerfield. The velocity v_N of the rocket at the end of burning of the last step is simply the sum of the contributions given in Eq. (117):

$$v_N = -Nc \ln[\epsilon(1-\lambda)+\lambda] - g_0 t_p, \qquad (118)$$

where t_p is the total firing time, and c is the average value of the exhaust velocity over the whole firing period.

The two important Eqs. (113) and (118) can be put in dimensionless form by introducing the over-all mass ratio G and the velocity ratio S, as follows:

$$G = M_0^{(1)}/M_l = \lambda^{-N}, \qquad (119)$$

$$S = (v_N + g_0 t_p)/c = -N \ln[\epsilon(1-\lambda)+\lambda]. \qquad (120)$$

The significance of these equations may be clarified by outlining the general approach to the problem of selecting a suitable step-rocket for a given mission. The nature of the mission determines v_N, and the propellant to be used determines the value of c, thus establishing the magnitude of the velocity ratio S. Prevailing experience in the design of structures usually indicates the magnitude of ϵ that may be assumed. With S and ϵ selected, various combinations of λ and N may be calculated from Eq. (120), and their simultaneous effect on the required mass ratio G follows from Eq. (119). In general, the larger the chosen number of steps, the larger is the payload ratio λ and the smaller is the required mass ratio G.

These effects may be examined quantitatively by reference to Figs. 53 and 54, where Eqs. (119) and (120) are plotted for several values of S and ϵ. Certain interesting generalizations about step-rockets may be observed.

(*a*) For a given mission, the mass of the required rocket is proportional to the mass of payload to be carried, despite the seeming insignificance of the payload in relation to the large mass of the rocket. If a payload of 100 lb requires a 10,000-lb rocket, a payload of 200 lb would require a 20,000-lb rocket.

(*b*) For given values of λ and ϵ, the number of steps required is directly proportional to the velocity ratio, and the required gross mass varies exponentially with the velocity ratio or the number of steps. Thus, for a given payload, if a three-step rocket weighing 1000 M_l is needed to achieve a speed of 25,000 ft/sec, then a six-step rocket weighing 1,000,000 M_l is needed to achieve a speed of 50,000 ft/sec.

(*c*) The required gross mass decreases as the number of steps is increased, but this decrease becomes less pronounced as the number becomes large. Mathematically it may be shown that if ϵ and S are held constant in Eqs. (119) and (120), G approaches an asymptote as N ap-

proaches infinity. Actually, the number N should not be chosen too large, lest the complexity and multiplicity of mechanisms cause an unfavorable increase in the structural factor ϵ. It seems, from inspection of Figs. 53 and 54, that a favorable combination of circumstances is obtained when λ lies between 0.20 and 0.40.

43. Numerical Examples of Multiple-Step Rockets

In SEC. **41** the energy required to escape from the earth was calculated, with reference to four specific missions. In SEC. **44** a method was presented by which the velocity at the end of burning of a step-rocket may be calculated. By carrying this analysis further, as in SEC. **40**, it is possible to derive a general expression for the altitude at the end of burning. An expression can then be written for the total energy of the vehicle at the end of burning. By equating the expression for the total energy to the required energy derived in SEC. **41**, the various characteristics of escape rockets can be computed.

A complete analysis of this kind is more complicated than is necessary for the purpose of this paper. However, an approximate expression for the total energy per unit mass of the vehicle can be obtained in the following manner. Assume that the powered portion of the flight is directed vertically upward with an *average* acceleration νg_0, and that the force of gravity is constant over the distance of powered flight. It can readily be derived that the gain in kinetic energy is equal to ν times the gain in potential energy. The increase in total energy may then be written

$$E=\frac{\nu+1}{\nu}[\tfrac{1}{2}(v_N+v_0)^2-\tfrac{1}{2}v_0^2], \qquad (121)$$

where v_0 is the initial, or launching, velocity of the first step and v_N is given by Eq. (118). By means of Eq. (121) the value of v_N required for a specific mission may be calculated.

The numerical examples in this section will be computed for the case $\nu=5$, although it is obvious that the precise acceleration is not critical as long as $\nu \gg 1$. Too large an acceleration must be avoided, however, lest the vehicle be damaged by high temperatures caused by atmospheric friction. In the first three cases discussed in SEC. **41**, the initial velocity v_0 is taken to be zero, since departure merely from the earth is considered. In the fourth case, that of departure from the solar system, v_0 is equal to the orbital velocity of the earth around the sun—98,000 ft/sec. The values of v_N calculated from Eq. (121), and the corresponding burning times and altitudes at the end of burning, are listed in Table X.

As a first example, an earth-satellite rocket is considered that utilizes the oxygen-hydrogen combination as a propellant. As in SEC. **42**, it may be assumed that the average exhaust velocity during powered flight can be as much as 12,500 ft/sec, which is about 20 percent more than the sea-level value listed in Table IV, and corrected in accordance with the footnote therein. Because of the low average density (14.8 lb/ft^3) of the two liquids, larger tanks, lines, valves, pumps, and so forth, are needed for a given weight of propellant than with denser propellants such as oxygen-ethanol. As a result, the structural factor ϵ would probably be larger than that of the V-2 rocket; a reasonable value is 0.33. The values of v_N and t_p are taken from the first line of Table X. Then, $c=12{,}500$ ft/sec, $g_0=32.2$ ft/sec^2, $t_p=150$ sec, $v_N=25{,}600$ ft/sec, and from Eq. (120), $S=(25{,}600+150\cdot 32.2)/12{,}500=2.44$.

Assume $N=4$ steps. Then, inserting $\epsilon=0.33$ in Eqs. (119) and (120), we find $\lambda=0.318$ and $G=98.0$.

Let the payload consist of instruments and a radio transmitter of combined mass 100 lb. It should be noted again that the mass of the required rocket is proportional to the mass of the

TABLE X. Characteristics of four escape missions.

Mission	Energy required for the mission (10^8 ft lb/slug)	Velocity[b] at end of burning (ft/sec)	Altitude at end of burning (mi)	Duration of powered flight (sec)
Earth-satellite at 200 mi (period, 1.5 hr)	3.56	25,600	200[d]	150
"Stationary" earth-satellite at 22,400 mi	6.29	32,500	620	200
Complete escape from the earth	6.79	33,700	670	210
Complete escape from the solar system[a]	54.8	39,000[c]	890	240

[a] The large energy corresponding to case (iv) is calculated with respect to the sun; the velocity is calculated with respect to the earth.
[b] The velocity at end of burning has been calculated on the assumption that the average acceleration of each step is approximately $5g_0$.
[c] To escape from the solar system the velocity of the rocket must be tangential to the earth's orbit to take advantage of the orbital velocity of 98,000 ft/sec.
[d] The powered-flight path of case (i) is not straight upward but curved to enter the orbit.

TABLE XI. Characteristics of eight escape rockets, with payload of 100 lb.

Mission	Propellant	No. of steps	Gross mass (10^6 lb)	Initial thrust (10^6 lb)	Est. length (ft)	Est. diam. (ft)
Earth-satellite	Oxy-Hyd	4	9.8	46.3	42	4.2
	Acid-An	6	54.4	241.0	52	5.2
Stationary satellite	Oxy-Hyd	5	35.4	160.0	64	6.4
	Acid-An	7	387.0	1670.0	100	10.0
Escape from earth	Oxy-Hyd	5	49.5	218.0	72	7.2
	Acid-An	8	425.0	2110.0	103	10.3
Escape from solar system	Oxy-Hyd	6	119.0	538.0	96	9.6
	Acid-An	8	2250.0	9420.0	178	17.8

payload. Therefore, in imagining such flights, one must think in terms of microinstruments, microradios and microcontrols. The gross mass of the rocket, from Eq. (113), becomes

$$M_0^{(1)} = 98.0 \times 100 = 9800 \text{ lb.}$$

The mass of propellant in the initial step is obtained by the use of Eq. (116), namely,

$$\zeta = (1-0.318)(1-0.330) = 0.457,$$

so that $M_p^{(1)} = 0.457 \times 9800 = 4480$ lb.

The assumption that ϵ, λ and ν are the same for each step, together with Eq. (96), requires that t_n, the burning time of any step, be equal to that of any other step. Therefore, the burning time of the first step is one-fourth of the total burning time of 150 sec; that is, $t_1 = 37.5$ sec.

The thrust of the first step becomes

$$F_1 = \frac{M_p^{(1)}}{t_1} c = \frac{4480 \times 12{,}500}{32.2 \times 37.5} = 46{,}300 \text{ lb.}$$

The acceleration at take-off is

$$a_1(\text{start}) = \frac{46{,}300 - 9800}{9800} = 3.7 g_0.$$

The acceleration at the end of the firing period of the first step is

$$a_1(\text{finish}) = \frac{46{,}300 - 5320}{5320} = 7.7 g_0.$$

The approximate size of the rocket may be estimated by first assuming a reasonable value for the average density of the rocket when loaded. Because of the very low density of hydrogen, the over-all density is taken here to be only 17.0 lb/ft^3. Then, taking the slenderness ratio (length/diameter) equal to 10, regarding the body of the rocket as a cylinder, and ignoring the volume and mass of the fins, one easily finds that the approximate dimensions are: length, 42.0 ft; diameter, 4.2 ft. Figure 51 is a schematic drawing of this four-step rocket.

In the same manner, the properties of other multiple-step rockets may be calculated. The essential characteristics of eight types are listed in Table XI. In each case, the payload is assumed to be 100 lb. The average density of all oxygen-hydrogen rockets is taken to be 17.0 lb/ft^3; that of nitric acid-aniline rockets 50.0 lb/ft^3. The structural factor ϵ is estimated to be 0.25 for all acid-aniline rockets and 0.33 for all oxygen-hydrogen rockets. A slenderness ratio of 10 is chosen for all cases. The average exhaust velocity of the acid-aniline rockets in flight is estimated to be 7600 ft/sec.

To summarize, it seems quite feasible from the engineering viewpoint to design and construct rockets that are capable of escaping from the gravitational field of the earth. By means of multiple-step rockets it is possible to achieve the tremendous velocities required without necessarily awaiting the development of fantastically light structures or nuclear-energy propulsion devices. The great importance of propellants having high exhaust velocities is to be emphasized, as well as the need for high density. Emphasis must also be placed on the lightening of structures and the careful, economical design of the payloads to be carried. But even these improvements are not absolutely necessary: the present state of rocket technology as embodied in the V-2 rocket is actually sufficiently advanced for the accomplishment of the task, insofar as propulsion is concerned.

Rockets Utilizing Nuclear Energy

44. Nuclear Energy

The advent of nuclear energy in technically usable amounts with its staggering million-fold

increase over the energy released in conventional chemical reactions has aroused especial interest in the application of this new technic to rockets. As pointed out in the introduction (SEC. 1), it is necessary in effective rocket design to reduce the amount of mass that must be ejected by the expenditure of tremendous amounts of energy. A rocket of 1000 lb thrust and 6200 ft/sec exhaust velocity operating at 40 percent thermal efficiency (this efficiency corresponds to 300 lb/in.^{2} chamber pressure for a rocket at sea level) consumes chemical energy at the rate of approximately 10,000 Btu/sec. A rocket of the same thrust and higher exhaust velocity will consume energy at the even higher rate

$$P = Fa/2\eta_i, \tag{122}$$

where P is the power, F is the thrust, a is the exhaust velocity,[40] and η_i is the thermal efficiency.

It would be out of place here to discuss nuclear reactions in detail; however, a very clear discussion of the general theory of nuclear reactions is given by Morrison,[41] and a concise account of "atomic" energy has been prepared by Tsien.[42]

Chemical energy results from the rearrangement of electrons (usually only the outermost electrons) as atoms come together to form molecules. The ionization energy of the hydrogen atom is about 13 ev and this may be taken as the order of magnitude of the energy involved in an elementary chemical process. Nuclear energy arises from the rearrangement of the protons and neutrons of atomic nuclei. Now the energy with which a single neutron or proton is bound to a nucleus is about 7×10^6 ev. We see at once why the nuclear reaction is so much more energetic than the chemical reaction.

Fission.—The nuclear reaction that first pointed the way to the technological use of nuclear energy was the fission of U_{92}^{235}. This reaction may be written as follows:

$n_0^1+U_{92}^{235}\rightarrow$Primary fission products$+180$ Mev; Primary fission products$\rightarrow$Final products$+20$ Mev.

We may note two facts about this process. First, the neutron that induces the fission of uranium does not have to carry any kinetic energy into the uranium nucleus. Merely inserting the neutron into the nucleus introduces an excitation energy of 6.4 Mev, and only 5.3 Mev is required for fission to be possible. This is important, because, it is only slow neutrons that have a large probability of being captured by the uranium nucleus. Second, the fission products are not unique, but are distributed over a variety of nuclear species which fulfill the requirements of conservation of charge and of mass number. Thus the primary products from the fission of one uranium nucleus might be $I_{53}^{141}+Y_{39}^{95}$, and from another nucleus they may be $2Pd_{46}^{118}$. Furthermore, the primary fission products are not stable, but have an excess of neutrons as compared to a "normal" nucleus of the same charge and so decay by neutron emission or electron emission until a stable nucleus is formed.

For definiteness we will write down the fission reaction that seems to be the most probable:

$$n_0^1+U_{92}^{235}\rightarrow(U_{92}^{236})$$
$$\rightarrow 2n_0^1+I_{53}^{140}+Y_{39}^{94}+179 \text{ Mev},$$
$$I_{53}^{140}\rightarrow Ce_{58}^{140}+5e_{-1}^{0}+17 \text{ Mev},$$
$$Y_{39}^{94}\rightarrow Zr_{40}^{94}+e_{-1}^{0}+4 \text{ Mev},$$

where the decay of I to Ce goes through the intermediate steps Xe^{140}, Cs^{140}, Ba^{140}, La^{140}. For calculation we will usually assume that

$$n_0^1+U_{92}^{235}\rightarrow 2Pd_{46}^{118}+170 \text{ Mev}$$

and

$$Pd_{46}^{118}\rightarrow n_0^1+Sn_{50}^{117}+4e_{-1}^{0}+15 \text{ Mev}.$$

On this basis the kinetic energy of a Pd nucleus (since conservation of momentum with equal masses implies equal distribution of energy) will be 85 Mev, and the particles will have a velocity of 1.2×10^9 cm/sec, or 3.94×10^7 ft/sec.

45. Relativistic Mechanics of Rockets

Ackeret[43] has discussed the application of relativistic mechanics to rockets. The velocity of fission fragments is generally less than 3×10^9 cm/sec, the velocity at which relativistic effects first become important. Nevertheless, it is interesting to see how the laws of relativity will modify the behavior of the "classical" rocket.

[40] Here we use a for exhaust velocity in order to save c for its time-honored role as a symbol for the velocity of light. They are not be confused with a for velocity of sound and c for effective exhaust velocity, as used in preceding sections.

[41] P. Morrison, *Am. J. Physics* 9, 135 (1941).

[42] H. S. Tsien, *J. Aero. Sci.* 13, 171 (1946).

[43] J. Ackeret, *Helv. Physica Acta* 19, 103 (1946).

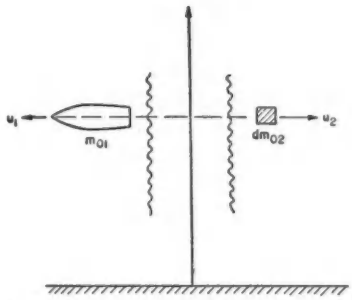

FIG. 55. Coordinate system and notation used in discussing the relativistic motion of a rocket. These axes are fixed relative to the *initial rest position* of the rocket.

Our investigation will also make clear-cut the domain in which classical theory may safely be used.

(i) *Static thrust.*—The usual relativistic definition of force as time-rate of change of momentum gives for the thrust of a rocket (in vacuum)

$$F=\dot{m}_0 a \tag{123}$$

where $\dot{m}_0$ is the rate of decrease of the *rest* mass of the *rocket* which is equal to the total rate of mass flow in the exhaust jet as seen from a system at rest with respect to the rocket. We note that if $\dot{m}_{0e}$ is the rate at which *rest* mass is carried off in the exhaust jet, then

$$\dot{m}_0=\dot{m}_{0e}(1-a^2/c^2)^{-\frac{1}{2}}. \tag{124}$$

Equation (123) is of precisely the same form and has the same significance as the thrust equation for a rocket in classical mechanics. The decrease of rest mass of the rocket is made up of two terms: (a) the amount of inert mass supplied to the exhaust jet $\dot{m}_{0e}$, and (b) the amount of mass "converted" (see reference 45, SEC. 46, subsection (iii)) into energy in order to accelerate the inert mass away from the rocket. Since it is the amount of mass the rocket must carry and not the amount of rest mass in the exhaust jet that is important in determining the performance of a rocket, Eq. (123) has the same significance as in the classical theory.

(ii) *The rocket equation.*—Although the static thrust equation is not modified by the application of relativistic mechanics to rockets, the over-all performance of the rocket is changed. The analysis is rather complicated, but Ackeret has hit on a clever scheme for calculating the final velocity of a rocket (as observed in a coordinate system in which it was initially at rest) in terms of the ratio ζ of propellant weight to initial gross weight and the exhaust velocity a relative to the rocket. This will now be presented.

Consider a rocket in empty space without the influence of external forces or fields. We will observe the motion from a system of coordinates in which the rocket was initially at rest. Let us assume that mass is ejected to the right with velocity u_2, and that the rocket proceeds to the left with velocity u_1. The *rest* mass of the rocket at any instant will be m_{01}, and the *rest* mass of a small portion of the exhaust jet will be dm_{02}, as in Fig. 55. For brevity, let the factors $(1-u_1^2/c^2)^{-\frac{1}{2}}$ and $(1-u_2^2/c^2)^{-\frac{1}{2}}$ be denoted by k_1 and k_2, respectively.

The equations of motion of the system are as follows: from conservation of energy,

$$\mathrm{d}(m_{01}k_1c^2)=k_2c^2\mathrm{d}m_{02}; \tag{125}$$

from conservation of momentum,

$$\mathrm{d}(m_{01}u_1k_1)=k_2u_2\mathrm{d}m_{02}; \tag{126}$$

from the relativistic formula for addition of velocities,

$$u_2=(a-u_1)(1-u_1a/c^2)^{-1}, \tag{127}$$

where a is the (constant) exhaust velocity relative to the rocket. Eliminating $k_2\mathrm{d}m_{02}$ between Eqs. (125) and (126), one obtains

$$u_2\mathrm{d}(m_{01}k_1)-\mathrm{d}(m_{01}k_1u_1)=0. \tag{128}$$

Eliminating u_2 from Eq. (128) by use of Eq. (127), we find

$$(a-u_1)\mathrm{d}(m_{01}k_1)+(1-u_1a/c^2)\mathrm{d}(m_{01}k_1u_1)=0. \tag{129}$$

This last expression may be simplified without too much difficulty to

$$\mathrm{d}m_{01}/m_{01}=-(1/a)\mathrm{d}u_1/(1-u_1^2/c^2). \tag{130}$$

Let v be the final velocity of the rocket. If W_p+W_0 is the gross weight of the rocket before firing and W_0 the final weight of the rocket after all the propellant has been ejected, then the ratio of propellant weight to initial gross weight is

$$\zeta=W_p/(W_0+W_p). \tag{131}$$

Integration of Eq. (130) then gives

$$(1-\zeta)=\left[\frac{c-v}{c+v}\right]^{c/2a}, \tag{132}$$

or

$$v/c=[1-(1-\zeta)^{2a/c}]/[1+(1-\zeta)^{2a/c}]. \tag{133}$$

This may be compared [see Eq. (101)] with the classical equation for field-free space,

$$v/a=-\ln(1-\zeta). \tag{134}$$

The general nature of these results is shown in Fig. 56. We see that the critical case, $\zeta=1$, corresponds to the velocity of light in the relativistic theory and to an infinite velocity in the classical theory.

Several things are noteworthy in Fig. 56. If $a=0.1c$, there is little difference between the classical and relativistic performance of rockets even when ζ is close to unity. For an exhaust velocity equal to $0.5c$, an appreciable difference between relativistic performance and classical performance occurs for mass ratios ζ as low as 0.5. The limiting case, $a=c$, the highest possible exhaust velocity allowed in the theory of relativity, has been shown in Fig. 56 since it may be of some interest. In general, the relativistic correction to classical theory is such as to reduce the terminal velocity of the rocket.

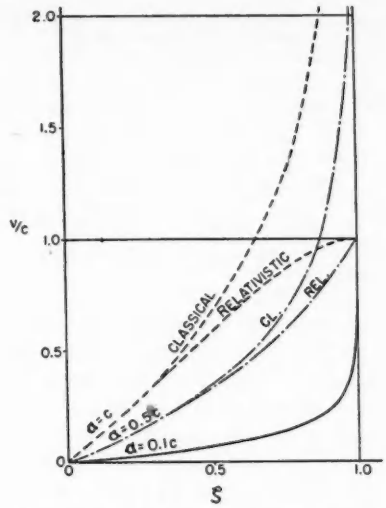

FIG. 56. The ratio v/c, where v is the terminal velocity of a rocket in field-free space, as a function of propellant mass ratio ζ.

46. Problems in the Utilization of Nuclear Energy

We have seen that the fission of U^{235} will produce particles of velocity 1.2×10^9 cm/sec. This is less than one-tenth the velocity of light, and we have seen that for such exhaust velocities classical mechanics is reliable up to mass ratios very near unity.

(i) *The ideal rocket.*—Assume that the rocket consists of a main body covered on the rear by a layer of fissionable material. To simplify the argument, assume that when the material reacts half of the particles go directly to the rear in the form of an exhaust jet and the other half go directly forward and transmit their momentum to the rocket. Since there is no known method of "reflecting" high-speed fission fragments, we must conclude that the forward-moving particles eventually stop in the main body of the rocket. We will assume that the mass of the main body of the rocket is large compared to the mass of propellant, which will shortly be seen to be true.

Conservation of momentum then gives

$$Mv=\tfrac{1}{2}ma, \tag{135}$$

where M is the mass of the main body, m is the total mass of fissionable material, v is the final velocity of the rocket, and a is the exhaust velocity—1.2×10^9 cm/sec in our example. Suppose we let M be 1 ton and v be the velocity necessary to escape from the earth, namely, 1.12×10^6 cm/sec. Then we find for m the value 3.75 lb. This is a small value, less than 0.2 percent of the mass of the rocket. Nuclear energy apparently reduces the magnitude of the mass ratio ζ required for a one-step rocket to escape from the earth from 0.96 for the hydrogen-oxygen chemical propellants to 0.00187 for uranium. However, there is a difficulty.

We recall that half the fission products were stopped in the body of the rocket, carrying momentum *and* energy. The energy that the rocket must absorb is

$$E=(\tfrac{1}{2})\tfrac{1}{2}ma^2. \tag{136}$$

For the values of m and a assumed, E is 5.5×10^{10} Btu. On the assumption that the rocket has the rather high specific heat of 1 Btu/lb °F, the rocket body will attain a temperature of 2.8×10^7 °F. A fantastic cooling problem! This situation

TABLE XII. Estimated performance of the radiating disk propelling a "photon rocket."

Pressure, p (lb/ft²)	Surface temperature, T (10^3 °K)	(10^3 °R)	Power radiated (Btu/ft² sec)	Weight-rate of flow (lb/ft² sec)
0.0532	10	18	2.22×10^5	5.3×10^{-9}
10	37	67	3.81×10^7	10^{-6}
100	66	120	3.81×10^8	10^{-5}
1000	117	210	3.81×10^9	10^{-4}
2120*	141	254	8.07×10^9	2.1×10^{-4}
10,000	208	375	3.81×10^{10}	10^{-3}

* Atmospheric pressure.

is aggravated when we realize that the primary fission products which have stopped in the body of the rocket are subject to radioactive decay with the release of approximately 10^9 Btu more. Of course, the actual temperature developed with an arrangement of this sort will be much less, since the specific heat of steel, for example, normally only 0.1 Btu/lb °F, will be greatly increased after the material is vaporized.

(ii) *Possible exhaust jets.*—We have just seen that the *direct* application of nuclear energy to rocket propulsion does not seem to be practical. This brings up the question of a suitable technic for application of this new energy. A good place to start is in the investigation of what will constitute a satisfactory exhaust jet. Assuming that nuclear energy can be controlled in a flexible manner, one may conceive of rockets utilizing photon (light) jets, jets of charged particles, or jets consisting of neutral particles.

(*a*) *The photon rocket.* A great deal would need to be done to develop intense sources of radiation before the photon jet would be practical. Although a low-thrust jet would enable a rocket ultimately to attain any desired speed in field-free space, near the earth the thrust would have to be at least as large as the weight of the rocket. A simple example makes the difficulty of this clear.

Suppose that the rocket "motor" consists of a layer of incandescent material at the rear of the rocket. As we will soon see, if excessive temperatures are to be avoided, this layer must be large in area, probably forming a disk many times the diameter of the rocket. We assume that the radiation leaves this disk uniformly in all directions within the solid angle 2π steradians at the rear of the disk. Suppose that in a thin layer next to the surface black-body radiation exists. The pressure p of this radiation will be given by[44]

$$p=\tfrac{1}{3}bT^4, \tag{137}$$

where T is the Kelvin temperature and the constant b has the value

$$7.62\times10^{-15}\ \text{erg cm}^{-3}\ \text{°K}^{-4}.$$

Now in this layer the density of energy ρ will be

$$\rho=bT^4, \tag{138}$$

and the energy will flow away from the rocket with the speed of light c. Consequently the power P radiated from unit area is

$$P=bT^4c. \tag{139}$$

The weight-rate of flow is obtained by dividing the power by c^2/g. The results of a calculation based on these equations are given in Table XII.

These results emphasize the tremendous power required to obtain a given thrust. For 1000-lb thrust, a photon rocket consumes 400,000 times as much power as an ordinary rocket utilizing chemical propellants [see Eq. (122)]. On the other hand, the mass flow of the photon rocket is greatly reduced, the specific impulse being 10^7 sec as compared to 200 sec for conventional rockets. Thus a given mass of "pure photon propellant" could support its initial weight in the gravitational field of the earth for 115 days as compared to 3.3 min for the usual chemical propellants.

The difficulty with the photon rocket is the large surface temperature needed to obtain a reasonable thrust from 1 ft² of radiating surface. A radiation pressure of only 10 lb/ft² requires a surface temperature of 37,000°K. To obtain atmospheric pressure, the temperature must be 141,000°K. The tungsten-filament lamp operates at a temperature of approximately 3000°K (tungsten melts at 3655°K). Suppose we are very optimistic and assume it will be possible to construct a radiating disk that operates at a surface temperature of 10,000°K. Using the corresponding radiation pressure in Table XII, we find that a surface area of 37,400 ft² is required to support 1 ton of rocket in the gravitational field of the earth. This is a circular disk 218 ft in diameter. It seems unlikely that a disk of such a large

[44] R. C. Tolman, *Relativity, thermodynamics, and cosmology* (Oxford, 1934), p. 139.

surface could be constructed to operate at high temperature and yet weigh less than 1 ton, and of course the rocket would also have to carry the power-generating apparatus and the payload. The "photon rocket" seems not likely to be practical in the near future.

(*b*) *Jets of charged particles.* In view of the excellent technics for accelerating charged particles that have been developed in recent years, a jet of electrons or protons with "exhaust velocities" approaching the velocity of light should be feasible. However, the rocket would rapidly acquire electric charge, and this would quickly stop a jet containing charge of only one sign. Perhaps positive and negative jets could be operated simultaneously. However, the energy required to separate the raw "fuel" into ions suitable for acceleration away from the rocket would be rather large, and this energy would be wasted. At the present time the intensity of the beams of charged particles from existing accelerators is far too small to furnish any appreciable thrust.

(*c*) *Jets of neutral particles.* The preceding discussion has narrowed down the practical technics for the application of nuclear energy to rocket propulsion to an arrangement in which a working fluid is heated by the nuclear reaction. This working fluid would then be used in the conventional exhaust nozzle to supply rocket thrust. Hydrogen might be a suitable working fluid because of its low atomic weight. The working-fluid technic would also make it possible to get rid of some of the radioactive products of the nuclear reaction by having them carried off in the exhaust jet.

(iii) *The technic of a working fluid.*—There is a fundamental difference between the conventional rocket utilizing chemical propellants and a rocket propelled by the combination of nuclear energy plus working fluid. A conventional rocket of high performance (for example, a rocket that is to leave the earth) must operate at a mass ratio ζ very near unity. In fact, for a single-stage rocket utilizing the very energetic hydrogen-oxygen chemical reaction this ratio must be 0.96 if the rocket is to escape from the earth. This mass ratio is so high that the construction of such a rocket is not practical. One then is forced to build a multistage rocket or seek even more energetic fuels. The fundamental cause of this difficulty is that the propellant is used both as a source of energy and as the mass to be ejected in obtaining thrust. It is true that one may add inert material to the exhaust jet, thus making energy supply and mass flow somewhat independent of each other; but since exhaust velocities obtainable from chemical propellants are not great enough for high-performance rockets, this is a purely academic procedure and is not used except in a very minor way to overcome cooling difficulties (see SEC. **32**).

With the system consisting of nuclear energy plus working fluid the situation is much different. It turns out that there is an optimum amount of working fluid, that too little working fluid is disadvantageous! This may be seen as follows. The fission reaction is so energetic that it represents, when compared to the chemical scale, almost pure "massless" energy. Thus, the energy released by 1 lb of U^{235} in the primary fission process is 3×10^{10} Btu, while the very energetic hydrogen-oxygen reaction generates only 6.85×10^{3} Btu/lb. We saw, in considering the "ideal" rocket, that only 3.75 lb of U^{235} was needed to project a 1-ton weight from the earth, and this is only 0.19 percent of the total mass of the rocket. Thus in our following considerations we will be quite justified in neglecting the mass of uranium as compared to the mass of the working fluid when considering mass flow. Of course, the mass of uranium will be important in considerations of energy supply and exhaust velocity.

Consider a rocket consisting of a payload of weight W_0, uranium of weight W_u, and working fluid of weight W_p. Except for the transformation of part of the mass of uranium into energy,[45] nonrelativistic mechanics can be used.

In a coordinate system at rest with respect to the rocket a small fraction δ of the mass of the uranium is converted into energy, $\delta W_u c^2$, given by the Einstein equation. This energy must be equal to the kinetic energy of the exhaust jet;

[45] This statement is not rigorously correct, but it serves to convey the essential notion without circumlocution. The correct statement is that part of the mass energy of the uranium is changed from mass, a sort of potential energy, into energy in a more useful form. Of course, the energy produced still has the same mass as it had before the transformation. No mass energy really appears or disappears. See E. F. Barker, *Am. J. Physics* **14**, 309 (1946).

that is,

$$\delta W_u c^2 = \tfrac{1}{2} W_p a^2. \tag{140}$$

For the uranium reaction, $\delta = 0.000731$; that is, less than 0.1 percent of the mass of uranium is converted into energy. We have assumed that there is no external pressure so that the exhaust velocity a corresponds to the full energy.

Neglecting the weight of uranium we can write the weight (or mass) ratio ζ as

$$\zeta = W_p/(W_0+W_p). \tag{141}$$

We may solve Eq. (140) for a and substitute this result and Eq. (141) into Eq. (134), to get the final velocity v of the rocket in field-free space; it is

$$v = -c(2\delta W_u/W_p)^{\frac{1}{2}} \ln\left(1-\frac{W_p}{W_0+W_p}\right),$$

or

$$v = c(2\delta W_u/W_p)^{\frac{1}{2}} \ln\left(1+\frac{W_p}{W_0}\right). \tag{142}$$

Now it happens that if W_0 and W_u are held fixed (fixed payload and fixed amount of energy), v has a maximum with respect to W_p at $W_p = 4W_0$. This is because W_p appears in the expressions for both the exhaust velocity (the square root) and the mass ratio. Near the maximum the curve is very flat, and any value of the mass ratio ζ larger than 0.5 gives a value of v close to the maximum. Assuming the 1 ton of payload and 3.75 lb of uranium found in the case of the "ideal rocket," and adding 4 tons of working fluid to the system, we find a final velocity v for the payload of 4.0×10^7 cm/sec, which is considerably larger than the 1.12×10^6 cm/sec attained without the use of working fluid.

It is interesting to note that the value of 0.8 for ζ which leads to maximum terminal velocity v is independent of the amount of energy supplied by the uranium (that is, of $\delta W_u c^2$) so long as W_u is small relative to W_0 and W_p. The assumption of small W_u is inherent in Eq. (141) and will always be valid so long as excessive temperatures have to be avoided.

The application of this optimum value of ζ is clearly limited to the hypothetical case in which the empty mass of a rocket is determined primarily by such factors as the reactor, the shielding, and the payload, the masses of which, it is presumed, cannot freely be altered by the designer. That is, the only important variable permitted the designer is the mass of propellant, and in this case, the optimum amount of propellant to be used is four times the given empty mass. If, on the other hand, the empty mass itself is subject to reduction by better design, each corresponding increase in ζ would result in an increase in velocity. Adjusting ζ back to the value 0.8 by removing propellant mass would, however, give still greater terminal velocity to the same empty mass, with no increase in amount of uranium required.

In the foregoing statements it is tacitly assumed that the propellant may be expelled at any temperature desired. It may be that some maximum allowable temperature must be selected for the working fluid, to avoid overheating the apparatus. This amounts to fixing the ratio of W_u to W_p in Eq. (142). The performance of the rocket is then quite conventional; that is, the final velocity increases steadily with increasing mass ratio, even beyond $\zeta = 0.8$.

It seems appropriate to indicate here what exhaust velocities might be obtained with the system of nuclear fission plus hydrogen gas as a working fluid, having regard for the limitations imposed by temperature. Taking into account the dissociation of hydrogen molecules into monatomic hydrogen and assuming that the exhaust is discharged into a vacuum, we may expect to obtain an exhaust velocity of 21,300 ft/sec if the gas can be heated to 4940°F, and 37,300 ft/sec if it can be heated to 10,340°F. This latter exhaust velocity is quite sufficient for a single-stage rocket to escape from the earth with a mass ratio of 0.5.

It is to be noted that the exhaust velocity increases more rapidly than $\sqrt{T}$. This favorable result is caused by the dissociation of the gas.

It is too early to say whether or not the problems resulting from the extreme temperature and radiation intensity accompanying nuclear reactions can be solved. Probably, in the near future, temperature will tend to limit the usefulness of nuclear energy applied to rockets. Dramatic improvements in performance over chemical rockets must await the invention of methods of applying nuclear energy that circumvent the temperature difficulty.

The Discovery of Convection Currents by Benjamin Thompson, Count of Rumford

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DURING his scientifically productive life, COUNT RUMFORD made many discoveries that profoundly affected the physical thought of his and succeeding generations. Tradition appears to dictate that his fame should rest most firmly on the one experiment described in "An inquiry concerning the source of the heat which is excited by friction."[1] The actual effect which this experiment had on the contemporary progress of science was considerably less than that of many others of his investigations. Of these latter, his research on "The propagation of heat in fluids"[2] was immediately recognized as a work of the first importance. His discovery of convection currents in the course of his studies on the propagation of heat is vividly described in his own words:

> In the course of a set of experiments on the communication of Heat, in which I had occasion to use thermometers of an uncommon size (their globular bulbs being above four inches in diameter) filled with various kinds of liquids, having exposed one of them, which was filled with spirits of wine, in as great a heat as it was capable of supporting, I placed it in a window, where the sun happened to be shining, to cool; when, casting my eye on its tube, which was quite naked, . . . I observed an appearance which surprised me, and at the same time interested me very much indeed. I saw the whole mass of the liquid in the tube in a most rapid motion, running swiftly in two opposite directions, *up* and *down* at the same time. The bulb of the thermometer, which is of copper, had been made two years before I found leisure to begin my experiments, and having been left unfilled, without being closed with a stopple, some fine particles of dust had found their way into it, and these particles, which were intimately mixed with the spirit of wine, on their being illuminated by the sun's beams, became perfectly visible, . . . and by their motion discovered the violent motions by which the spirit of wine in the tube of the thermometer was agitated. . . . On examining the motion of the spirits of wine with a lens, I found that the ascending current occupied the axis of the tube, and that it descended by the sides of the tube.
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> . . . . . .
>
> It was by the motions of the very fine particles of dust, which by accident had been mixed with the spirits of wine in my large thermometer, . . . that I discovered the internal motions in that Fluid which take place when it is cooling; and, availing myself of this kind hint, I contrived to render the internal motions of water equally visible. This I immediately saw could be done with the utmost facility if I could but find any solid body of the same specific gravity as water, . . . that would not be liable to be dissolved by it. . . .
>
> Not being able to find any solid substance fit for my purpose, of the same specific gravity as pure water, I was obliged to have resource to the following stratagem.
>
> Looking over the tables of specific gravities, I found that the specific gravity of transparent yellow amber was but a little greater than that of water; . . . and it occured to me, that, by dissolving a certain quantity of pure alkaline salt, I might augment its specific gravity, or rather bring the specific gravity of the solution to be precisely equal to that of amber. . . .
>
> This contrivance was put into execution in the following manner, with complete success. Having provided myself with a number of glass globes of various sizes, with long cylindrical necks, I chose one which was about 2 inches in diameter, with a cylindrical neck $\frac{3}{4}$ of an inch in diameter, and 12 inches long; and putting into it about half a teaspoonful of yellow amber, in the form of a coarse powder, . . . I poured upon it a certain quantity of distilled water. . . . Finding, as I expected, that the amber remained at the bottom of the globe, I now added to the water as much of a saturated solution of pure vegetable alkali as was sufficient to increase the specific gravity of the water (or rather of the dilute saline solution), till the pieces of amber began to float, and remained apparently motionless in any part of the liquid where they happened to rest.
>
> . . . . . .
>
> I had now an instrument which appeared to me to be well calculated for the very interesting experiments I had projected, and it will easily be imagined that I lost no time in making use of it.
>
> The first experiment I made with this instrument was to plunge it into a tall glass jar nearly filled with water almost boiling hot. . . . Two currents, in opposite directions, began at the same instant to move with great celerity in the liquid in the cylindrical tube, the ascending current occupying the sides of the tube, while that which moved downwards occupied its axis.
>
> As the saline liquor grew warm, the velocity of these currents gradually diminished, and at length, when

[1] *Phil. Trans.* **88**, 80 (1798).

[2] Rumford, *Essays, political, economical, and philosophical* (T. Cadell and W. Davies, London, 1798), Essay VII, vol. II, p. 197. This essay is reprinted in the *Complete works of Count Rumford* (American Academy of Arts and Science, Boston, 1870), vol. I, p. 237.

the liquor had acquired the temperature of the surrounding water in the jar, these motions ceased entirely.

On taking the glass body out of the hot water, the internal motions of the liquor recommenced, but the currents had changed their directions, that which occupied the axis of the tube being now the ascending current.

. . . . . .

The immediate cause of the motions in a liquid, which take place on its undergoing a change of temperature, is evidently the change in the specific gravity of those particles of the liquid which become either hotter or colder than the rest of the mass. . . . The more a liquid is expanded by any given change of temperature, the more rapid will be the ascent of the particles which first receive the Heat; and as these are immediately replaced by other colder particles, which, in their turns, come to be heated, this must of course produce a rapid communication of Heat from the hot body of the liquid.

. . . . . .

This fact will, I flatter myself, be considered as affording the most unquestionable proof that could well be imagined, . . . that Heat is propagated in water only in consequence of the motions which the Heat occasions in the insulated and solitary particles of that fluid."

COUNT RUMFORD published his Seventh Essay, from which the foregoing quotations have been extracted, in 1797. The very important effect which it had on the natural philosophy of his day may be easily verified by anyone who cares to browse through chemistry and physics textbooks published during the succeeding 30 or 40 years. Almost without exception one finds either that RUMFORD is credited directly with having discovered the propagation of heat in fluids by means of the internal motion of their particles, or the less direct but just as sure indication of his influence in the description of the demonstration of the effect by particles of amber suspended in a dilute saline solution.

The effect went without a name for many years. It was not until 1834 that the well-known WILLIAM PROUT, writing in one of the *Bridgewater Treatises*, suggested:[3]

> There is at present no single term in our language employed to denote this mode of propagation of heat; but we venture to propose for that purpose the term *convection* (*convectio*, a carrying or conveying), which not only expresses the leading facts, but also accords very well with the two other terms [conduction and radiation].

Even after this name was suggested, one finds that it was another 20 years before *convection* found its way to the universal acceptance which the term now enjoys.

This discovery of COUNT RUMFORD'S was well appreciated by his contemporary scientists and was almost immediately incorporated into the thoughts and writings of the time. It might be pointed out that this was in rather sharp contrast to the experiments on the energy concept of heat by which RUMFORD is best known in the present day. His friction experiments were considerably ahead of the popular thinking of his time and were not generally accepted until about 50 years after they were performed.

[3] *Bridgewater treatises* (W. Pickering, London), vol. 8, p. 65. Volume 8, which was published in 1834, was written by Prout.

Meeting of the Chicago Section

The Chicago Section of the American Association of Physics Teachers met on February 15, 1947. The program, dealing with the Annual Meetings of the Association and the American Physical Society in New York, follows:

General papers of the joint meetings of the two societies. M. N. STATES, *Central Scientific Company.*

Highlights of the Physical Society meeting. H. M. SULLIVAN.

Committee on premedical physics. L. A. BOCHSTAHLER, *Northwestern University.*

Highlights of the Association meeting. M. A. COUNTRYMAN, *Illinois Institute of Technology.*

Business matters of the Association. C. J. OVERBECK, *Northwestern University.*

CARL A. BENX, *Secretary*

The Science Training Group

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THE shortage of scientific personnel has made it increasingly imperative to provide facilities for the additional education of workers in government laboratories. Recognizing the need for a study of these problems in the area of Washington, D. C., the interdepartmental Advisory Committee on Scientific Personnel[1] on January 15, 1946 organized the Science Training Group (STG). Its stated function is "to evolve and carry out a program for facilitating and encouraging the further training of scientific personnel at the graduate level." The STG is a portion of a long-range objective to make government service more attractive to scientific and technical personnel. The reluctance of many scientists to accept and remain in government service has become an increasing handicap to the extensive program of research and development that is being promoted by government laboratories, especially those connected with the armed forces. To assist in providing opportunities for professional growth, the STG encourages attendance in graduate courses of study in the educational institutions in the Washington area.

The chief functions of the Group have been (*a*) to survey the needs of government workers in the Washington area for graduate study in the natural sciences, (*b*) to assist in the organization of new courses and programs of informal in-service training at the graduate level and (*c*) to assist in coordinating the work being offered in various graduate schools so as to insure the maintenance of high academic standards and facilitate the interchange of credits for courses taken in various institutions.

The membership of the Science Training Group is composed of representatives from many of the governmental agencies in the Washington region, including such departments as Agriculture, Commerce, Federal Security, Army and Navy. Some 25 scientists are included in the list of official representatives; a number of others have participated actively in various of the functions of the group.

The first officers of the Science Training Group were: Marsh W. White (War Department), *Chairman*, and Philip N. Powers (Navy Department), *Executive Secretary*. In November 1946 the following new officers assumed their duties: *Chairman*, L. A. Sandholzer (Fish and Wildlife Service); *Vice Chairman*, Heinz Specht (National Institute of Health); *Secretary*, Harold F. Eisele (Agriculture).

To establish policy and report on the progress of its subcommittees, the group holds regular monthly meetings, with special meetings called as necessary. Much of the detailed work of the group during its first year was carried on in the Office of the Executive Secretary with the assistance of Keith C. Harder and John Harms, of the Office of Naval Research.

Committees of the STG have been working on a number of projects, including the following subjects: establishment of graduate courses, counseling of scientific personnel, laboratory facilities for thesis work in the Washington area, in-service training and lecture courses, standards for graduate courses, and training needs in the biological sciences.

The STG has made two extensive surveys of needs for graduate courses in the Washington area in an effort to determine the extent and character of the educational facilities that should be provided for the training of government scientists. In January 1946 the requirements of about 600 prospective graduate students were analyzed, and in September 1946 some 1000 individuals furnished data concerning needs for graduate courses. It is believed that more than 10,000 people in the Washington area who are working for government agencies are more or less potentially interested in further training programs at the graduate level. Data indicate that the STG has been effective in developing a coordination of effort and stimulation of interest in further graduate study in more than 700 scientists in government laboratories who are currently enrolled in graduate courses in the Washington area. About 26 graduate courses during the current semester have been sponsored by the STG for scientists and engineers to study

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[1] M. H. Trytten, "The Advisory Committee on Scientific Personnel," *Science* 103, 437 (1946).

after working hours in locations at or close to their place of work.

A general objective of the STG is to make the Washington area a center of scientific education comparable to its importance and scope in the field of scientific research and development. Much effort is being expended in promoting cooperative relationships between the local graduate schools in such matters as transfers of credit, utilization of government laboratory facilities for thesis work, the accrediting of off-campus courses and the elimination of duplicate programs.

Each participating agency has been encouraged to set up an Advisory Committee on Scientific Education and Training in order to provide more accurate information as to the types of courses needed by their scientists and to encourage and advise individual workers with information as to the planning of a complete graduate program of study. Students are referred to local heads of departments of the sciences and to the deans of graduate schools for specific information as to requirements for degrees and local regulations concerning course and thesis requirements. The departments of Agriculture, Commerce, Navy and War have been particularly active with these advisory committees.

One example of the services of the STG to Washington scientists is the leaflets which were prepared and circulated to about 6000 people, listing all of the graduate courses in the sciences offered by the various institutions in and near Washington. These leaflets were circulated before the summer and fall semesters of 1946. They listed the course numbers, titles, credit hours, times and locations of the work being offered, together with condensed information concerning registration, costs and sources of further information.

Institutions participating in the work of the STG include the U. S. Department of Agriculture Graduate School, the Catholic University of America, Georgetown University, George Washington University, Howard University, the University of Maryland and the National Bureau of Standards Graduate School. The cooperation of the University of Maryland and the U. S. Department of Agriculture Graduate School has been particularly effective.

A novel feature of the program of the STG is its Standards Committee. This is composed of well-known scientists familiar with academic problems and includes the following members: L. A. Sandholzer (Fish and Wildlife Service), M. A. Mason (Beach Erosion Board), E. H. Kennard (David Taylor Model Basin), F. G. Brickwedde (National Bureau of Standards), and Philip N. Powers (Office of Naval Research). This Standards Committee (*a*) critically analyzes the data showing needs for various courses so as to reduce the number of requests for courses to the graduate schools; (*b*) assists in the location of suitable teachers upon the request of the participating universities and reviews the qualifications of all instructors of courses organized with the help of the STG; (*c*) checks the prerequisites of the course and the qualifications of the students, to assist in the maintenance of high academic standards, particularly for off-campus courses; (*d*) reviews the progress of the course and the achievements of the students with the objective of evaluating the quality of the work so that all courses certified by the committee will be acceptable for transfer of credit by leading graduate schools. The Standards Committee attempts to work closely with the participating departments and graduate schools by means of agreements arrived at with the responsible heads of the institutions.

The functions of the STG are admittedly in the experimental stage. Additional fields of activity other than organized graduate courses are under advisement. The procedures developed by the group in the Washington area may be of value in other sections of the country where there are large concentrations of government scientific and technical personnel. It is hoped that these additional educational and professional advantages will assist materially in making government service attractive to scientists.

The truth is never simple, and rarely pure.—OSCAR WILDE.

Reproductions of Prints, Drawings and Paintings of Interest in the History of Physics

31. The Scientific Paintings of Joseph Wright

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THE three great scientific pictures—"The Orrery," "The Air Pump," and "The Alchymist"—painted by JOSEPH WRIGHT (1734–1797) deserve to be better known to teachers and students of science than they are, for they accurately, as well as beautifully, reflect the state of physical science during the artist's lifetime. "WRIGHT of Derby," as he is often called, was not only a distinguished artist especially noted for his rendering of artificial lighting effects, but he was also "an interested and careful student of contemporary science whose knowledge of the scientific details portrayed in his pictures was surprisingly accurate."[1] In a very real sense "the three pictures may be regarded as typical of the state of astronomy, physics, and chemistry respectively in the latter half of the eighteenth century."[1]

The scientific accuracy and interest of these three great paintings have been so happily discussed by F. W. SHURLOCK that I can do no better than to refer the reader to his delightful paper.[1] Any student who will study the paintings under the guidance of this paper will acquire a new insight into the background and spirit of 18th science.

The reproductions for this article were made from the beautiful mezzotint engravings of the original paintings which were executed during WRIGHT'S lifetime by WM. PETHER, VALENTINE GREEN and T. BOYDELL. These engravings are approximately 23×18 in. in size and are among the finest examples of the mezzotint art. The mezzotint process, with its affinity to painting in black and white, is well adapted to the reproduction of the unusual lighting effects that are a

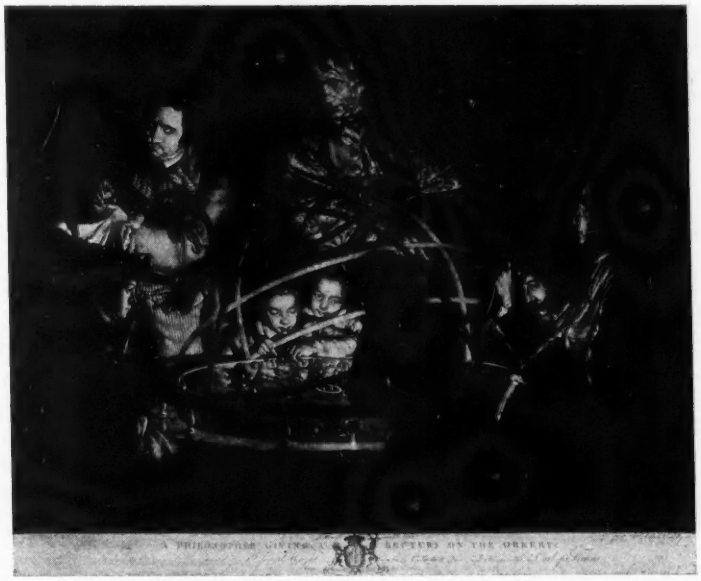

PLATE 1. A philosopher giving a lecture on the orrery.

[1] F. W. Shurlock, "The scientific pictures of Joseph Wright," *Sci. Progress* **17**, 432 (1923).

PLATE 2. A philosopher shewing an experiment on the air pump.

PLATE 3. The alchymist, in search of the philosopher's stone, discovers phosphorus, and prays for the successful conclusion of his operation, as was the custom of the ancient chemical astrologers. [Courtesy of D. D. Berolzheimer.]

characteristic feature of WRIGHT'S work; but of course it can give no hint of the taste and skill with which the artist "united the blazing reds and yellows of the central glare to the rich brown of his transparent shadows, warmed and cooled these shadows with gleams of red coat and glimmers of blue sash and white dress, and led the eye, untired, from the ruddy glow of the chamber to the cool night outside."[2]

[2] W. Bemrose, *The life and works of Joseph Wright, A.R.A., commonly called "Wright of Derby"* (London, 1885).

There is no more terrible sight than ignorance in action.—GOETHE.

NOTES AND DISCUSSION

A Stereoscopic Effect of Snow Sparkles

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IT was a clear winter day with a brilliant sun shining in a cloudless sky on a freshly fallen mantel of snow. Looking at the snow surface toward the sun from a standing position I noticed that many of the bright sparkles of light glinting from the snow crystals appeared in space above the surface, giving an enchanting stereoscopic effect. "Sparkle" is perhaps not the exact word, for it connotes a flickering or varying light; whereas the little points of light shining from the snow were quite steady, as long as one's head was steady. There were roughly about two sparkles per square inch on the snow surface and about five or ten sparkles per cubic foot in the first foot above the surface; there were none above about 3 ft.

The stereoscopic effect was due to the fact that each eye saw a different pattern of sparkles on the snow surface; this was determined by first closing one eye and then the other. When the two patterns were fused in binocular vision the space effects appeared. If, for example, there were two sparkles, say 3 in. apart, the right-hand sparkle being seen only by the left eye and the left-hand sparkle only by the right eye, and if the lines of sight from each eye to its respective sparkle intersected, the sparkle seen binocularly appeared in space at the intersection of the two lines. Tilting or moving the head, or walking, caused the space pattern of the sparkles to change. Later, when the snow melted slightly, the sparkles lost some of their original brilliancy and the stereoscopic effect was not easily seen. Further observations after other falls of snow indicated that the stereoscopic effects were at their best when the sparkles were most brilliant and not too close together. This occurred with fresh dry snow and a clear day. Probably the effect could be demonstrated in the laboratory by means of crystals scattered not too copiously on a black cloth and illuminated by a bright, nearly a point, source of light.

A Simplified Technic for Studying the Inclined Plane

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Hunter College, New York 21, New York

THE common procedure in studying the inclined plane as a machine is to measure the force necessary to slide or roll a load up the plane without acceleration. This is usually done by attaching one end of a cord to the load and passing the cord, which must be parallel to the plane, over a pulley at the upper end of the plane. Weights are fastened to the free end of the cord and varied until the load, once set in motion, moves up the plane with constant speed. This procedure is not easy for students inexperienced in manipulating laboratory equipment and is open to errors in judging constant speed.

A simpler technic is to move the plane instead of the load. This may be done by using two inclined planes in contact, a movable one on top of a fixed one, the load being placed on top of the movable plane. The load is kept at rest by means of a spring balance fastened to the top of the fixed plane so that the spring-balance force is parallel to the plane. When the movable plane is moved downward and equilibrium has been reached, the corrected spring-balance reading is the reaction force to the sum of the frictional force between the load and the plane and the component of the weight of the load down the plane, and is independent of the motion of the movable plane. This procedure therefore eliminates the need for judging constant speed. Hence, the corrected spring-balance reading is equal to the force that would move the load up the plane without acceleration. If the load is sliding instead of rolling on the movable plane, it is necessary that the surfaces in contact be uniform in order to get a constant reading of the spring balance when the plane is being moved downward.

This procedure calls for very simple manipulation, and the time required to obtain the data is lessened considerably.

A Mercury Spectrum Source

M. W. SCHWINN
Oberlin College, Oberlin, Ohio

A CONVENIENT, inexpensive and economical spectral line source of medium intensity can be obtained through the use of a type 816 mercury-vapor rectifier operated entirely from 115 v a.c. For the operation and use of this tube as a spectral source there are required a filament transformer, a time-delay element, a series resistor, and a suitable mounting.

Figure 1 shows the schematic wiring diagram. The 816 tube was chosen because of its small physical size and the

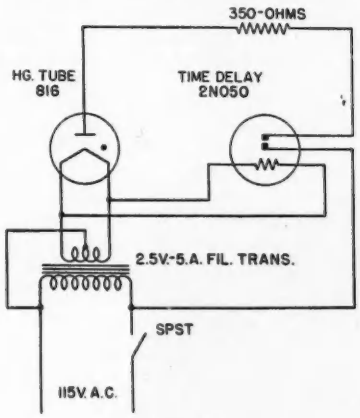

FIG. 1. Wiring diagram.

moderately low filament current requirement. An 866A tube was used in the first unit with equally good results. It is necessary to choose a tube in which the glow discharge is not entirely shielded from view at right angles to the cathode-anode path. The filament transformer must furnish 2.5 v at a minimum of 2 amp for the 816 tube. The 5-amp requirement indicated in Fig. 1 is necessary if the 866A tube is to be used. The time-delay element is used to give the filament of the 816 tube time to reach operating temperature before the plate voltage is connected. The 816 tube requires at least 10 sec, and hence a 2N010 tube[1] will suffice (10-sec delay). A longer time provides a factor of safety. A 2N050 tube (50-sec delay) is shown in Fig. 1. The 350-ohm, 10-w series resistor is used to limit the plate current to the recommended value.

The mounting shown in Fig. 2 is made of sheet iron. The front and slotted top (one piece) and the bottom are removable to provide convenient access to all parts. The time-delay tube and the series resistor are mounted in the base along with the filament transformer. This base is spotwelded in place about 1 in. from the bottom of the mounting. The 10-mm rod is also bolted to this base and is given rigidity by passing through an angle bracket just below the top. Since the placement of the anode and the cathode shield vary from tube to tube, it is necessary to make the side opening in the light shield large enough to allow for tube difference. The light shield is adjustable for height and held in position by a setscrew. Even with a simple amount the cost of the essential elements of this source will probably be less than the cost of the mounting.

In addition to the regular mercury lines present, most tubes show three red lines as indicated in Table I. This is a great advantage in providing additional data for dispersion curves for prisms. In some cases these lines grow dim or disappear after protracted use each time the tube is put in operation.

TABLE I. Wavelengths (mm) of the lines.

0.000630 (approx.)	0.000546
0.000613 (approx.)	0.000436
0.000604 (approx.)	0.000408
0.000578 (av.)	

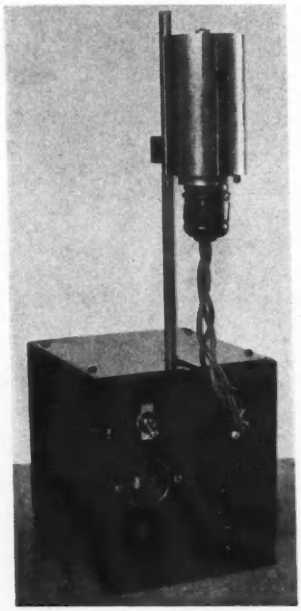

FIG. 2. Photograph of complete source.

I am indebted to Dr. C. E. Howe for the suggestion that led to the development of this simple mercury source.

[1] Obtainable from the Amperite Company, 561 Broadway, New York, 12, New York.

Conservation of Energy and Mass; Arguments in Favor of Discarding the Quantity "Relativistic Mass"

AUSTIN J. O'LEARY
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THERE is no clear-cut distinction in Newtonian dynamics as to which of several quantities—mass, weight, force and momentum—is a primary quantity and which a secondary quantity; the very meaning of a primary quantity is left obscure. And in setting up the restricted theory of relativity, the usual procedure has been to modify classical mechanics without considering the question of primary and secondary quantities. It is my purpose to present a new view of rest mass and relativistic mass based on a restatement of the basic relations of dynamics in relativistic form with particular attention to the way in which a third primary quantity may be defined with reference to a standard. The approach is an objective one initiated long ago by Mach and which I have developed further in a recent paper.[1]

(*1*) *Laws and definitions.*—Letting γ denote the relativistic correction factor $(1-v^2/c^2)^{-\frac{1}{2}}$, we have, for any particle x in an isolated system of n particles:

$$\frac{d}{dt}(\mathbf{v}_x\gamma_x)=\sum_{y=1}^{n}\frac{d}{dt}(\mathbf{v}_x\gamma_x)_y, \quad (y\neq x) \tag{1}$$

where $d(\mathbf{v}_x\gamma_x)/dt$ denotes the *effective* time-rate of change of $\mathbf{v}_x\gamma_x$, and $d(\mathbf{v}_x\gamma_x)_y/dt$ the *individual* time-rate of change of $\mathbf{v}_x\gamma_x$ in an interaction between particle x and any other particle y of the system;

$$\frac{d}{dt}(\mathbf{v}_x\gamma_x)_y\Big/\frac{d}{dt}(\mathbf{v}_y\gamma_y)_x=-k_{y-x}, \tag{2}$$

where k_{y-x} is a positive numeric; and

$$k_{y-x}=k_{y-s}/k_{x-s}, \tag{3}$$

where, for the time being, s denotes any third particle of the system. The three complementary sets of relations, Eqs. (1), (2) and (3), express a law arrived at by generalization from a large accumulation of measurements. I shall not take space here to develop the generalization; anyway, we often reverse historic procedure and simply state a law subject to *a posteriori* confirmation.

From the correlation between the numerics k, Eq. (3), we perceive the convenience and simplification to be gained by the following procedure. We imagine the International Kilogram to be one of the particles of the system and designate it as a standard particle s; k_{x-s} is characteristic of particle x, and k_{y-s} is characteristic of particle y, both relative to the standard s. We replace the numerics k by a primary physical quantity expressed in specific units. Calling this quantity *mass* and denoting it by m', we have, by definition, $m_s'=1$ kg, $m_x'=k_{x-s}$ kg, $m_y'=k_{y-s}$ kg. . . . The measurement of particular values of m' on the spring balance and beam balance, and by various kinetic methods, may easily be established from Eqs. (1), (2) and (3).

The secondary quantity $m'\mathbf{v}\gamma$ is called the *mechanical linear momentum* of a particle, by definition. Combining Eqs. (2) and (3) with the definition of m' and adding an electromagnetic term for the case of an interaction between charged particles[2] (the electromagnetic term is of the second order and may be neglected in most calculations), we get

$$\dot{\mathbf{p}}_{x-y}+\dot{\mathbf{G}}_{x-y}=-(\dot{\mathbf{p}}_{y-x}+\dot{\mathbf{G}}_{y-x}), \tag{4}$$

where $\dot{\mathbf{p}}_{x-y}$ and $\dot{\mathbf{G}}_{x-y}$ are the individual time-rates of change of mechanical and electromagnetic linear momentum, respectively, of particle x in its interaction with particle y; $\dot{\mathbf{p}}_{y-x}$ and $\dot{\mathbf{G}}_{y-x}$ refer similarly to particle y. Setting $\mathbf{F}=\dot{\mathbf{p}}+\dot{\mathbf{G}}$, by definition, Eq. (4) takes the form

$$\mathbf{F}_{x-y}=-\mathbf{F}_{y-x}.$$

Various quantities are defined with dimensions $[ML^2T^{-2}]$ in such a way as to satisfy a law of conservation of energy. Or, to put it another way, we have been able to define different physical quantities representing individual forms of energy such that the total energy of an isolated system is conserved in all known instances, where, by definition, *total energy* is the sum of all the energy quantities of various types identified with the system.[3] The following are examples pertinent to the present discussion.

The quantity $m'c^2(\gamma-1)$ is called the kinetic energy of a particle, by definition; denoting it by $\mathcal{E}$, we have

$$\mathcal{E}=m'c^2(\gamma-1)\equiv m'c^2\left(\frac{1}{2}\frac{v^2}{c^2}+\frac{1\cdot3}{2\cdot4}\frac{v^4}{c^4}+\frac{1\cdot3\cdot5}{2\cdot4\cdot6}\frac{v^6}{c^6}+\cdots\right).$$

The quantity $m'c^2$ is called the *rest energy* of a particle (*inherent energy* might be more appropriate since m', and therefore also $m'c^2$, is defined for all particles independently of speed); denoting it by E, we have, by definition, $E=m'c^2$; or, in the form generally used in applications,

$$\Delta E/\Delta m'=931.4 \text{ Mev amu}^{-1}. \tag{5}$$

In radiation, the quantity $h\nu$ is called the *energy of a photon*. What we are saying is essentially this: experiment indicates that there is a unique value of the Planck constant h such that, in all instances involving radiation, a law of conservation of energy is satisfied when $h\nu$ is taken to be a particular energy quantity by definition.

The quantity $E+\mathcal{E}$, or $m'c^2\gamma$, I shall call the *inertial energy* of a particle (would the term *relativistic energy* be more appropriate?). Whatever we choose to call it, a special name for this quantity is a great convenience in describing nuclear, atomic and molecular changes.[4] All such changes may be described as changes in individual amounts of inertial energy, frequently accompanied by a transformation of inertial energy into radiant energy, or *vice versa*, so that the total energy is conserved in all instances; thus we have

$$\Delta(\Sigma E+\Sigma\mathcal{E}+\Sigma h\nu)=0. \tag{6}$$

(2) *Conservation of mass.*—As we may see by dividing Eq. (6) by c^2, the question of conservation of mass[5,6] is purely one of the definition of total mass, that is, whether or not the total mass of a system is taken equal to $\Sigma m'+\Sigma\mathcal{E}/c^2+\Sigma h\nu/c^2$. In my opinion, calling these three different quantities *mass* (with, of course, appropriate qualifying terms as in the case of different forms of energy) introduces a source of confusion without contributing anything toward clarification or simplification in our thinking.

Let us first consider $\mathcal{E}/c^2$—*kinetic mass*, I suppose we should call it. Inclusion of kinetic mass as a component of total mass alters the classical significance of mass to no useful purpose. It is surely no aid in our thinking to describe the absorption of high-energy particles in terms of a transfer of kinetic mass from one particle to another as the system tends toward equilibrium; similarly for conditions following the absorption of radiation. Also, in view of the high precision with which $\Sigma m'$ may be measured, it is unsatisfactory to add to $\Sigma m'$ a term $\Sigma\mathcal{E}/c^2$ which is largely indeterminable for a system of molecules; only $\Delta(\Sigma\mathcal{E}/c^2)$ is accurately measurable. Furthermore, the value of $\Sigma\mathcal{E}/c^2$ for a system depends on how we treat the system, that is, whether we consider it from the macroscopic point of view or in terms of the elementary particles comprising it; as Deutsch[5] expresses it, "except for bodies at a temperature of absolute zero, as far as mass is concerned, the whole [mass of an entire body] is less than the sum of its parts [masses of the individual particles composing the body]!"

Going one step further and assigning mass $h\nu/c^2$ to a photon[7] would only tend to obscure in our minds a desirable distinction between matter and radiation; similarity in the properties of matter and radiation is already fully brought out in the laws of conservation of energy and of momentum. The main purpose in so extending the definition of mass would seem to be to state the law of conservation of energy in an alternative way as a law of conservation of mass. The tendency seems to be to remove all distinction between mass and energy; for example, we find statements such as this:[8] "Since mass and energy are proportional, it is quite immaterial whether reaction energies are measured in calories, or in grams, per mole." That is not so; it is not dimensionally correct to express energy in grams. It would seem preferable to distinguish clearly between energy and mass, as in the following application of Eqs. (5) and (6) to the reaction $Li^7+H^1\rightarrow He^4+He^4$:

$$\Delta m'=2(4.0039)-(7.0180+1.0081)$$
$$=-0.0183 \text{ amu};$$
$$\Delta E=-17.0 \text{ Mev, from Eq. (5)};$$
$$\Delta(h\nu)=0;$$
$$\Delta\mathcal{E}=+17.0 \text{ Mev, from Eq. (6)};$$

therefore,

$$\mathcal{E}_{(He^4+He^4)}=17.0\ \text{Mev}+\mathcal{E}_{(H^1)}.$$

(3) *Suggestion to discard the quantity "relativistic mass."*—In current usage, $m'\gamma[\equiv m'+\mathcal{E}/c^2]$ is called the relativistic mass of a particle, or simply its mass m; and the quantity here denoted by m' is called rest mass and is usually designated by m_0, its name and its symbol both implying zero speed of the particle. Whatever reasons there may have been for these conventions in 1905, it would now seem advisable to revise them. In particular, it is so when dynamics is viewed from the approach of this paper. The arguments may be summarized as follows:

(i) m' is defined on a basis of Eqs. (1), (2) and (3), for particles having any speeds up to the limiting value c. The name "rest mass" is inappropriate. The mere fact that m' is most commonly measured on a beam balance does not in any way change the definition of m' for all particles whether in motion or at rest.

(ii) As may be seen from SEC. (*1*), a special name for $m'\gamma$ is wholly unnecessary in developing relativistic dynamics. It would be far more useful to have special names for $\mathbf{v}\gamma$ and $d(\mathbf{v}\gamma)/dt$.

(iii) The quantity $m'\gamma$ has no worth-while significance by itself; it appears only as an integral part of quantities containing other factors—for example, momentum $m'\mathbf{v}\gamma$, in which γ is associated with $m'\mathbf{v}$, not with either m' or $\mathbf{v}$ separately; in fact, if one were to insist on separating $m'\mathbf{v}\gamma$ into two parts, it would be natural to keep $\mathbf{v}$ and γ together as in Eqs. (1) and (2). Concerning the definition of momentum in the restricted theory of relativity, one may simply say it is defined as a different function of velocity than in classical theory.

(iv) Employing the name "mass" for the secondary quantity $m'\gamma$ is a complication and source of confusion, as indicated in SEC. (*2*).

I think we should, even at this late date, attempt to restrict application of the name "mass" to the primary quantity m', the one significant quantitative measure of "amount of matter"; we would, of course, denote mass by m in place of m' or m_0. If we do not revise the current definition of mass, we should at least recognize that the variation of mass with speed in relativity theory is not something that enters more or less automatically in the extension of classical dynamics, as is often implied; it is purely the result of a particular choice in the definition of mass, and the desirability of the current definition is highly questionable.

[1] A. J. O'Leary, *Am. J. Physics* **15**, 146 (1947).

[2] See A. J. O'Leary, *Am. J. Physics* **14**, 63 (1946); also reference 1, p. 150.

[3] I think we sometimes forget that different energy quantities are defined for the specific purpose of attaining a law of conservation of energy. The "law" resides in the experimental fact that we have always been able to find such quantities whenever new situations have arisen in the last century. Mechanical momentum of a particle, electromagnetic momentum of a particle and momentum of a photon are similarly defined in such a way as to satisfy a law of conservation of momentum. The underlying purpose is sometimes not altogether clear from the way in which the definitions are arrived at. For example, A. Einstein [*The meaning of relativity* (Princeton, 1945), p. 44] introduces dynamics with this statement: "We shall assume that the principles of momentum and energy are valid for the body." Succeeding definitions of momentum and energy are then made to conform with this assumption, or rather, this purpose.

[4] These changes are often described as "a conversion of matter into energy," and *vice versa*. Such usage is questionable on several grounds: (i) the counterpart of matter is radiation, not energy; (ii) this use of the word "matter" implies an explicitly defined physical quantity expressed in specific units, which might better be called "amount of matter" or "mass." But even if the word "mass" is substituted for "matter," the description is not improved much; the units of mass and energy have different dimensions and are not commensurable.

[5] C. R. Eddy, "A relativistic misconception," *Science* **104**, 303 (1946); also comments by M. E. Deutsch, V. P. Barton, and A. J. O'Leary Science **104**, 400 (1946).

[6] E. F. Barker, "Energy transformations and the conservation of mass," *Am. J. Physics* **14**, 309 (1946).

[7] In a somewhat more conservative view, it has been customary to call $h\nu/c^2$ the *equivalent mass* of a photon. The equivalence of $h\nu/c^2$ pertains, of course, to $m'+\mathcal{E}^e/c^2$, not to m' alone. Hence if $\mathcal{E}^e/c^2$ is not accepted as a component of total mass, equivalent mass of a photon loses its meaning.

[8] Reference 6, p. 310.

Proceedings of the American Association of Physics Teachers

Report of the Secretary

The Executive Committee of the American Association of Physics Teachers held its annual meeting at the American Institute of Physics, New York City, on January 30, 1947. President R. C. Gibbs presided. The committee was in session from 7:30 P.M. to 12:30 A.M.

Members of the Association who attended were: Mildred Allen, *D. M. Bennett, L. I. Bockstahler, *T. B. Brown, *M. A. Countryman, H. L. Dodge, J. D. Elder, *R. C. Gibbs, *R. C. Hitchcock, H. K. Hughes, *P. Kirkpatrick, K. Lark-Horovitz, *R. B. Lindsay, W. H. Michener, *T. H. Osgood, *C. J. Overbeck, F. Palmer, *R. F. Paton, *W. B. Pietenpol, *D. Roller, *H. K. Schilling, S. S. Sidhu, *O. H. Smith, R. J. Stephenson, M. N. States, R. M. Sutton, *L. W. Taylor, M. H. Trytten, *B. B. Watson, A. G. Worthing. Asterisks indicate the names of those serving on the Executive Committee as members or proxies for this meeting. The others were present by invitation.

Business with American Institute of Physics.—Our 1947 contribution as a Member Society in support of the Institute is again fixed at 15 percent of the dues collected from individual members in 1946. M. W. White was elected to serve, with P. E. Klopsteg and R. C. Gibbs, as representative of the Association on the Governing Board of the Institute for a three-year term.

Reports of officers.—Each officer presented a brief report of the work for the year. The Secretary announced that an "anonymous friend of the Association" has agreed to pay for certificates to be issued to recipients of the Oersted medal.

The Journal.—Upon recommendation of the editor, P. Epstein, H. A. Nye and H. K. Schilling were appointed

associate editors for the three-year period, 1947–1950. The number of pages to be published this year will be approximately 25 percent greater than that published in 1946.

Committee on Membership.—L. W. Taylor reported 277 new members in 1946 and approximately an additional 200 new members to date in 1947. The rolls of the Association carried 1442 names as of December 15, 1946.

Budget Committee.—The Executive Committee voted to accept the budget for 1947 set by its Budget Committee as follows: Membership Committee, $150.00; Program Committee, $50.00; Committee on Teaching of Physics and the Physical Sciences in Secondary Schools, $500.00; travel expenses, representatives to ACE, $100.00; services of Assistant Editor, $600.00; Editor's office expenses, $1200.00; Secretary's and Treasurer's office expenses, $700.00.

Other Committees.—Written or brief oral reports were presented by the chairmen of other committees. Several of these committees indicate that the results of their work may soon appear in print. Several new problems have been assigned to committees for study. These include: (*a*) revision of our present Emeritus Membership plan; (*b*) life memberships; (*c*) classification of physicists *versus* methods of accreditation of department; (*d*) sponsoring the production of motion-picture films and lantern slides; (*e*) procedures for nomination of officers.

Regional Sections.—All ten sections of the Association have filed their annual reports with the secretary. Four new regions have requested information on organization.

Future meetings. The next meeting of the Association will be in cooperation with the American Society for Engineering Education (formerly SPEE). The present plans are to hold this meeting at the University of Minnesota, June 18–21, 1947.

Annual business meeting.—The sixteenth annual business meeting was held on February 1, 1947, at 9:30 A.M. The secretary reported on the work of the Association and its executive committee. Major items of the Treasurer's Report were read. The teller committee report on the outcome of the annual election was presented. The 565 identified and counted ballots resulted in the election of:

President: PAUL KIRKPATRICK.
Vice President: J. W. BUCHTA.
Secretary: C. J. OVERBECK.
Treasurer: P. E. KLOPSTEG.
Executive Committee: MILDRED ALLEN; H. K. SCHILLING.

The terms of the President and Vice President are one year; those of the remaining officers, two years.

The secretary presented the recommendation of the Executive Committee that, in view of increased costs of publication and the anticipated extension of the *American Journal of Physics* from six to nine issues in 1948, the dues be raised, on January 1, 1948, from $5.00 to $6.00 per year for Members, and from $2.50 to $3.00 per year for Junior Members. The secretary moved that this recommendation be accepted. The motion was seconded and unanimously passed.

C. J. OVERBECK
Secretary

Annual Report of the Treasurer

Balance brought forward from December 15, 1945		$5127.72
CASH RECEIVED		
Dues received from members	$6493.65	
Dues received for 1945	30.00	
Dues received for 1947	502.50	
Dues received for 1948	15.00	
Royalties, *Demonstration experiments in physics*	110.25	
Constituent Membership in ACE, paid by American Institute of Physics	100.00	
Bond coupons	85.00	
Total deposited, 12/5/45 to 12/15/46		7336.40
Total cash available		$12,464.12
DISBURSEMENTS		
Postage and supplies, editor's office	168.00	
Postage and supplies, treasurer's office	67.00	
Secretary's office expenses	208.30	
Stenographer, editor's office	742.80	
Salary of assistant editor	300.00	
Travel expenses, editor and assistant editor	320.73	
Expenses of membership committee	164.93	
Expenses of nominating committee	21.97	
Printing	415.85	
Payments to the American Institute of Physics	2152.95	
Constituent membership in ACE	100.00	
Miscellaneous travel expenses	130.38	
Bank expenses, discount on checks, etc.	10.35	
Miscellaneous	27.25	
Total disbursed		4830.51
Balance on hand December 15, 1946[1]		$7633.61

PAUL E. KLOPSTEG, *Treasurer*

I have audited the books of account and records of Dr. Paul E. Klopsteg, Treasurer of the American Association of Physics Teachers, for the year ended December 15, 1946, and hereby certify that the foregoing statement of receipts and disbursements correctly reflects the information contained in the books of account. Receipts during the year were satisfactorily reconciled with deposits as shown on the bank statements, and all disbursements have been satisfactorily supported by vouchers or other documentary evidence. U. S. Government Bonds and Notes of $5000 par value are held in safekeeping by the State Bank and Trust Company of Evanston, Illinois, and a certificate was obtained from the custodian as of December 15, 1946.

WILLIAM J. LUBY
Certified Public Accountant

Evanston, Illinois,
January 20, 1947.

[1] Approximately $1000 is still due the American Institute of Physics for publication of the journal for 1946. In addition to the balance indicated, the Association holds U. S. Government Treasury Bonds and Notes of par value $5000.

New Members of the Association

The following persons have been made *members* or *junior members* (*J*) of the American Association of Physics Teachers since the publication of the preceding list [*Am. J. Physics* **15**, 79 (1947)].

Adams, Charles P., Box 20, Emory University, Ga.
Adel, Arthur, 1018 Vaughn St., Ann Arbor, Mich.
Albright, Penrose S., 220 N. Terrace Drive, Wichita 8, Kan.
Ames, Bill Chester (*J*), 7504 Nestle Ave., RFD, Reseda, Calif.
Ammerman, Charles, Jr. (*J*), 860 Big Bond Road, Kirkwood 22, Mo.
Antonakos, Antonios, Savannah Div., University of Georgia, Savannah, Ga.
Azbell, William, 200 S. Douglas, Peoria, Ill.
Bailey, Frank, 1397 Coventry St., Akron 1, Ohio.
Belai, Louise G., 102 Millville Ave., Hamilton, Ohio.
Benumof, Reuben, 10 Monroe St., New York 2, N. Y.
Bernard, C. H., A & M College of Texas, College Station, Tex.
Binford, Frederick H., 807 Walker Ave., Memphis 6, Tenn.
Byers, C. C., Oregon State College, Corvallis, Ore.
Byers, Walter H., University of Oklahoma, Norman, Okla.
Campbell, Clarence W., Flint Junior College, Flint 3, Mich.
Carney, Rose A., St. Xavier College, 4928 S. Cottage Grove, Chicago, Ill.
Charles, Sister Mary, Immaculate Heart College, Los Angeles 27, Calif.
Cobas, Amador, University of Puerto Rico, Rio Piedras, Puerto Rico.
Coker, R. L., 1877 Corden Ave., Memphis 4, Tenn.
Compton, Dale (*J*), Wabash College, Crawfordsville, Ind.
Copperman, Reuben (*J*), 2525 W. Columbia Ave., Philadelphia, Pa.
Cudney, James A. (*J*), 700 S. Harvey Ave., Oak Park, Ill.
Cunningham, Frank W., 227 C, NE, Miami, Okla.
Decker, Fred W., Oregon State College, Corvallis, Ore.
Dempster, Richard R., 505 N. 3 St., Corvallis, Ore.
Dick, E. G., 321 S. Willow, Ottawa, Kan.
Doll, Alfred Wm., 115 Dogwood Ave., Melverne, L. I., N. Y.
Dyer, Wilbur E., 50 Cherry Hill Road, Joliet, Ill.
Earl, E. Philip, 15297 Lanning Ave., Lakewood 7, Ohio.
Edwards, Donald A., Lincoln University, Jefferson City, Mo.
Erickson, Leland (*J*), 2128 S. E. Hawthorne, Portland 15, Ore.
Erickson, Willard L., 1054 9 St., Boulder, Colo.
Fallon, Leslie D., Geneva College, Beaver Falls, Pa.
Ferguson, Jeremiah M., 259 Main St., Hornell, N. Y.
Fesmire, Warren C. (*J*), Wheaton College, Wheaton, Ill.
Fink, Harold O., 1006 University, Carlinville, Ill.
Garrett, Milan W., 336 N. Princeton Ave., Swarthmore, Pa.
Giles, Frederick H., Jr. (*J*), 252 S. Central Ave., Marshfield, Wis.
Gould, Theodore, Kentucky State College, Frankfort, Ky.
Graves, Leon F., University of Houston, Houston 4, Tex.
Groenier, Willis L., 217 E. Marquette Road, Chicago, Ill.
Gustafson, Arthur G., 710 Jefferson Ave., Seguin, Tex.
Hahn, Thomas M., Jr., 9614 51 Ave., Berwyn, Md.
Hardin, Luther M., Morgan State College, Baltimore 12, Md.
Hayes, John L., Lew Wallace School, Gary, Ind.
Hays, Earl E. (*J*), 2410 R. R. 1, Des Plaines, Ill.
Holzer, Robert E., Pomona College, Claremont, Calif.
Hratz, Joseph A., St. Ambrose College, Davenport, Iowa.
Huff, L. D., Box 1423, Clemson, S. C.
Iloff, Philip M., Chico State College, Chico, Calif.
Jackson, Herbert A., 204 Fifth Ave., SE, Minot, N. D.
Jacobs, James A., State University of Iowa, Iowa City, Iowa.
Jensen, Harald C., Lake Forest College, Lake Forest, Ill.
Jones, I. L., 2127 Derby St., Berkeley 5, Calif.
Kalmbach, S. H., U. S. Naval Academy, Annapolis, Md.
Kalos, Malvin H. (*J*), 63–30 Fitchett St., Forest Hills, N. Y.
Kane, Brother Gabriel, Manhattan College, New York 63, N. Y.
Karen, Abraham, 1724 Crotona Park East, New York 60, N. Y.
Kaufman, Raymond, 1590 E. 172 St., New York 59, N. Y.
Kenworthy, Ray W., University of Washington, Seattle 5, Wash.
Kirsch, Simon C., Woodstock College, Woodstock, Md.
Kissenger, Charles W., 2905 Carleton Ave., NE, Washington 18, D. C.
Kuester, Erhard J., Quincy College, 1831 College Ave., Quincy, Ill.
Laurence, James C., 1203 Pitkin Ave., Akron 10, Ohio.
Lee, Konrad O., Concordia College, Moorhead, Minn.
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